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CONTENTS

No. 1, March, 1938

No. 2, June, 1938

No. 3, September, 1938

No. 4, December, 1938

VOL. 13, NO. 1 March, 1938

THE QUARTERLY REVIEW of BIOLOGY

WHAT ARE THE GENES?

I. THE GENETIC AND EVOLUTIONARY PICTURE

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INTRODUCTION

Genes as particulate bodies

MODERN studies in more than one branch of biology have brought into the foreground an intricate but not incomprehensible organized process underlying the phenomena of heredity.

There are five fairly obvious modes of scientific study of these phenomena: Experimental genetics, cytology of germ cells, chemical investigation of the constitution of nuclei, studies in evolution, and investigation of the developmental processes through which the genetic potentialities come into active expression. Each of these approaches has been used by scientists—the first two most intensively—and many valuable results achieved, especially where more than one of the methods has been brought to bear on a single aspect of the problem. The present paper and its sequel endeavor to place the problem of genes briefly under combined illumination from the diverse slants of cytology, genetics, biochemistry, and portions of the evolutionary field of study.

In the interpretation of the empirical data of heredity a leading rôle has been played by the concept of genes, or minute organized units of material located in their respective chromosomes, which exert a regulating influence on all the formative processes by which the new individual comes to express the characteristics of its species and variety. In its present form this concept comes jointly from experimental studies in breeding and from a close microscopical examination of the chromosomes and the transformations that they undergo (Morgan, 1928, etc.). In this way the particularistic nature of the hereditary process has been thoroughly verified, and the broader features have been determined in the mechanics of the distribution of the hereditary particles during the formation of the sex cells and their union into zygotes. Each unit of heredity, or gene, has its own distinct and peculiar influence upon the heritable characteristics of the whole organism. They are contributed to the zygote in essentially equal numbers by each gamete or sex cell, so that (except for the special case of X-

and Y-chromosomes, that are involved in the determination of sex) the hereditary influences of the male and the female parent are in the long run essentially comparable and evenly balanced (Jollos, 1935). This gives us confidence that genes belong in the chromosomes, which alone are equally represented in the germ cells of both sexes, and that they do not reside in the cytoplasm or in the yolk substance, since these latter are contributed by the female without seriously altering the balance of hereditary transmission. The motile flagellum or tail of the spermatozoon is also denied any serious rôle in the particulate determination of heredity, since it has no counterpart in the female, and the process by which it originates is unsuitable for producing the specialized assortment of genes which is revealed by the Mendelian laws.

Present-day geneticists are reasonably united in the conviction that the particularistic gene mechanism is concerned with virtually 100 per cent of what is heritable. This rather sweeping conclusion is the gradual outcome of a long search for evidence. The most effective argument from experiment is based on a comparison of inbreeding and outbreeding (Fisher, 1930). If heredity depended to any marked extent upon a matrix capable of being subdivided and reblended (as is implied in the ancient concept of ancestral "blood") then cross-breeding would have a powerful averaging effect, with an end product almost as uniform as that produced by inbreeding. But experiment reveals a degree of contrast between in- and outbreeding which points to a wholly or almost wholly particularistic background for heredity. (Green, 1933; Castle, *et al.*, 1936.) Hence the modern chromosomal-Mendelian concept is usually believed to apply to essentially the entire field of heredity in higher organisms.

The main outlines of the chromosomal theory and the arguments for belief in such "genes" as it postulates, have been covered with such care by so many authors, that it will be well for us to omit the greater part of their discussion and proceed after merely rehearsing certain conclusions which we take for granted as already scientifically established.

The genes are material bodies occurring in a linear sequence in their respective chromosomes.

The unfertilized sex cell carries a single series of genes which cover the whole range of heritable traits. The chromosomes carrying this series constitute what is called the "haploid" count.

Through the mechanics of chromosome distribution the different portions of this series are derived from various ancestral sources by a special kind of lottery.

The sexual union of the gametes endows the fertilized ovum and each of the resultant body cells with two such series. These cells then possess two *simple* or haploid counts making one "diploid" or *double* count of chromosomes amounting in all to a double set of the gene mechanism.

Within each of these two series, reduplications seem to be the exception rather than the rule for animals, although in higher plants there is often copious reduplication.

The serial order of genes is relatively fixed in any one species, and experimentally determinable. Alterations of this order, when they occur, are also experimentally verifiable.

The continuity of genes through the cell generations must be postulated to account for the retention of both normal and atypical serial arrangements.

Every part of this gene assemblage is indispensible, in the sense that if a fertilized ovum carries a simultaneous shortage of appreciable blocks of corresponding genes in its two series, such a double defect has always proved to be fatal.

The distribution of chromosomes is such that typically every chromosome furnished to the organism is distributed to all cells of the body, so that the genes it contains are given ubiquitous opportunities for exerting their influence.

Hereditary differences may doubtless sometimes consist in the presence or absence of single genes or pairs of genes, but they are more often referable to alterations of the constitution of some gene, such

that it becomes endowed with a different degree (or type?) of potency.

A change in the number of genes present of a single kind is at times indubitably the source of outstanding qualitative bodily differences. By far the most important example of this is the differentiation of sexes. In numerous groups of animals there is a block of genes—the "X" component, frequently present as a specialized chromosome—which determines development as a female if it is present in duplex, while if it occurs but once it is unable to inhibit the male developmental trend that is normally present in all cells. A few groups of animals have the reverse relation of chromosomes in the two sexes.

A differing space-arrangement of genes does not usually exert more than a trifling effect on somatic characters, although it may heavily disorganize the mechanics of sexual reproduction.

GENES IN MUTATION

From the standpoints of phylogeny, evolution and paleobiochemistry, one of the most important qualities of the gene mechanism is its liability to mutation, although from the detailed standpoint of the genetic relation between parent and offspring every occurrence of a mutation must be understood as representing some sort of mishap. The frequency of such genetic accidents may be increased experimentally by applying appropriate irradiation to the gonads. Within our theoretical pattern the mishaps may be of three possible types, either (1) a faulty distribution of the chromosomal material, so that certain blocks of genes are either omitted or reduplicated in the resultant germ cells, or (2) a rearrangement of the series, bringing certain genes into new juxtapositions which may supposedly condition some degree of change in the net efficiency of their mutual functions, or (3) some sort of qualitative or quantitative alteration of a single gene, without radical alteration of the general mechanism of the germ cell.

The fact has already been mentioned that changes in the *number* of genes may alter the character of the organism, and the mechanism of sex-determination has been cited in illustration. Many families of plants give fine illustrations of "mutations" that consist in the possession of redundant chromosomes, thus demonstrating that quantity differences in the equipment of genes will alter the visible traits of the species.

Accidental redundancies of this sort can provide a source for progressive increase in the genetic machinery beyond what was ancestrally present in the stock, but it obviously produces no gene of any new kind.

Mutations of the second type, by rearrangement without loss or addition, is of relatively minor importance, although throwing a light upon the manner of action of genes (Dubinin and Sidoroff, 1934).

Mutations of the third type, involving changes limited to single genes, have much the greatest interest for our problems. The greater part of the vast array of known Mendelian factors in animals is ascribed to this class. There is an almost universal tendency for new mutations of the sort found in the laboratory to be recessive as compared to the norm of the species. Since the effect of a gene and of its recessive allelomorph is usually after the manner of "all or nothing," a single dominant gene being approximately as effective as a pair, the relationship has commonly been interpreted in the past in terms of "presence" and "absence," a recessive being looked upon as the mere omission of a gene which when present is equally potent in single or double dose. There then follows Bateson's (1914) paradoxical corollary that observed mutations consist exclusively (or almost exclusively) in a process of eliminating genes from the germinal lineage, and, if we are rigorously logical, that the entire process of evolution of life from its beginning on this planet

can be described at least formally as a shedding of gene after gene.

Today it probably will seem more rational to assume simply that impairments are so much more easily produced than new or augmented potentialities through the agency of genic mishaps that the former have thus far overcrowded the field of laboratory observations and stood in the way of finding and studying the more significant positive alterations. If, for example, a gene is momentarily retarded in its process of self-reduplication, a daughter cell must result from which that particular gene is absent. Or if perhaps some slip has occurred in the chemical sequence, and the gene comes to balance on a slightly altered chemical constitution, the gene, by analogy with enzymes, is vastly more likely to suffer an impairment than an augmentation of its catalytic powers. (Johnston and Winchester, 1934.)

A single gene, or gene locus, may be the site for a whole series of genetic alterations, or "mutations," which are all allelomorphic or alternative to each other. It has been found that the mutant forms of organism produced by such a series of allelomorphic genes always differ from each other in degree rather than in kind, showing various levels of accentuation of the respective character, rather than any different assemblage of characters. Shall we draw the conclusion that the gene differences themselves are differences in quantity rather than in kind or nature? Such has actually been inferred, and again has been doubted as a conclusion exceeding the evidence, and by others has been either partially contradicted or even discarded for the completely reverse theory. (Goldschmidt, 1927, 1935a and b; Hammerschlag, 1935; Stern, 1929; Morgan, 1928; Demerec, 1933.)

In this connection one of the most intensively discussed cases of gene modifications is the "Bar" series in *Drosophila*, discovered by Morgan and largely studied in his laboratories. (Hersh, 1934.) This gene locus is found in the X-chromosome and affects the shape and the number of facets in the fruit-fly's eyes.

If we give the symbol B to the original normal gene in the "Bar" locus, there are two major variants known, which can most conveniently be called B_1 (infrabar) and B_2 (bar). Any one of these three acts as allelomorph to either of the others, with the heterozygotes showing more or less intermediate states. But in addition, when cross-over takes place near these genes, they are liable at times to cross unevenly, and in this manner strains have arisen having two of the genes in a single chromosome, either B_1B_1, B_2B_2, B_1B_2 or B_2B_1. The two latter, being quantitatively equal and only different in their relative map positions, do not need separate discussion. Combining two X-chromosomes in one female fly, there results the observed set of genic constitutions with the corresponding average facet numbers characteristic of each (Table 1). In this table the symbol $B{:}B_1$ means that one of this fly's chromosomes is normal, the other has an infrabar gene; while $B_2{:}B_1B_2$ indicates one chromosome carrying bar, the other carrying both bar and infrabar.

The first point to note in this table is that the character is determined in the majority of cases by the total list of genes rather than by their manner of grouping. $B_1B_2{:}B_1B_2$ is the same as $B_1B_1{:}B_2B_2$, except, of course, that the latter produces offspring in a heterozygous, the former in a homozygous manner. Similarly, $B_2{:}B_1B_1$ produces the same somatic result as $B_1{:}B_1B_2$. Only in the combinations $B_1{:}B_2B_2$ and $B_2{:}B_1B_2$ does a slight difference appear. One hesitates to judge how much weight to give to this rather small differ-

ence, whether it should count as indicating that differences of location are not always completely without effect, or whether it is merely one of those deviations that will occur in a relatively short string of statistics. In any case the proposition still holds, that nature and count of genes signify more than their positions.

Goldschmidt (1927) has suggested that the numerical relations between these heritable effects are best understood as functions of the cell divisions leading to the formation of the embryonic mother cells of the ommatidia. This is accomplished if we express the numbers as powers of two, to correspond to the number of cell generations. Thus 2^7 or 128, represents 7 generations of cells before facet formation commences. It then becomes obvious that there is a heavy spread of the genic effect between the figures 2^6 and 2^8, indicating that this portion of the curve is modified, perhaps by a time factor, perhaps by a differing grade of physiological susceptibility. This skew in the curve causes B_2 to appear as almost a dominant in relation to B_1, although it is imperfectly recessive to B. From observations of this type Goldschmidt has constructed a powerful argument for interpreting dominance and recessiveness as representing the effects of speed factors and physiological transition phases in the embryological development, rather than qualitative presence and absence. This mode of interpretation is extremely valuable, and there are cases in which its correctness cannot be doubted. Morgan has published convincing evidence that certain of the differences found in the bar series are quantitative, while other differences he is inclined to interpret as qualitative. Goldschmidt on the other hand argues for a quantitative explanation of the entire series, and sees in it a reinforcement of his emphasis upon widespread quantity differences in genes. It is verified by their genetic origin, that double infrabar, B_1B_1 and double bar, B_2B_2 are quantitative increases (doublings) respectively of infrabar and bar. To a biochemist it does not necessarily follow from this that all of the series differs exclusively in the quantities of genic material.

TABLE 1

B : B	779 (normal)	$B_2 : B_2$	68	$B_2 : B_1B_2$	37
B : B_1	716	B : B_1B_2	50	$B_2 : B_2B_2$	36
B : B_2	358	B : B_2B_2	45	$B_1B_1 : B_1B_2$	28
$B_1 : B_1$	320	$B_1 : B_2B_2$	42	$B_1B_1 : B_2B_2$	27
B : B_1B_1	200	$B_2 : B_1B_1$	38	$B_1B_2 : B_1B_2$	27
$B_1 : B_1B_1$	138	$B_1 : B_1B_2$	38	$B_1B_2 : B_2B_2$	24
$B_1 : B_2$	73	$B_1B_1 : B_1B_1$	38	$B_2B_2 : B_2B_2$	24

Analogies from the endocrine and pharmacological fields indicate that we can ascribe the result equally well to qualitatively modified molecules in some of these genes, leading to speed differences in the physiological catalytic effect. Furthermore, some of the numerical relations almost defy interpretation in terms of a single quantitative variable. This difficulty is best shown in the relations between B_1 and B_2. In the six instances in the tabulation in which B_2 becomes substituted for B_1 and produces a significant result, the differences between the B_1 effect and the B_2 effect are shown in Table 2. A comparison of the fifth entry in this table with the two preceding, and of the fourth entry with the sixth, reveals a

heavy discrepancy in the amount of alteration occurring when a B_2 is put in place of a B_1. This is suggestive of something qualitatively different in the nature of these genes, since the divergences are difficult to explain if B, B_1, and B_2 are merely different definite quantities of the same gene substance.

The picture presented by the Bar series thus contains features of superlative interest to any who would trace the evolutionary possibilities of the gene mechanism. A taxonomist would, to be sure, view this as a case of retrograde evolution, not of philogenetic progress. But genetically we find a reasonable argument that one gene has been replaced by two, both of which differ from their antecedent in their qualitative chemical constitution, and hence that a genuine progression has occurred from a less complex to a more complex hereditary machinery. It corresponds exactly to the type of change which the evolutionary biologist must believe has occurred millions of times, leading up by successive steps from the simple nuclear mechanism of a primitive organism to the complexities that the higher plants and animals now display. If we are correct in imagining that certain of these gene variations are chemically qualitative, it is only necessary to add the postulate of a regulative principle, such as natural selection, to rationalize the observed evolutionary sequence of progressive adaptation in terms of the mechanics of the chromosomes and the chemical mutability of the particles known as genes. The test of the scientific value of this qualitative chemical interpretation of mutation depends upon its future utility as a framework for further biochemical and genetic research.

An additional argument in favor of the qualitative nature of some mutations can be drawn from the reciprocal relations between groups of mutations that are all referable to one gene locus. So long as mutations are mere alterations in the effective mass of a gene, they will lie in a single, unbranched, ladder-like series for each gene, thus:

TABLE 2

	GENETIC CONSTITUTIONS COMPARED	PHENOTYPES		NUMERICAL DIFFERENCES	CHANGE IN POWER OF 2
(1)	$B : B_1$ *vs.* $B : B_2$	716	358	358	1.0
(2)	$B_1 : B_1$ *vs.* $B_1 : B_2$	320	73	247	2.1
(3)	$B : B_1B_1$ *vs.* $B : B_1B_2$	200	50	150	2.0
(4)	$B_1 : B_1B_1$ *vs.* $B_1 : B_1B_2$	138	38	100	1.85
(5)	$B_1 : B_2$ *vs.* $B_2 : B_2$	73	68	5	0.1
(6)	$B : B_1B_2$ *vs.* $B : B_2B_2$	50	45	5	0.15

$$A \rightleftharpoons B \rightleftharpoons C \rightleftharpoons D \rightleftharpoons E \rightleftharpoons F$$

it being understood that either one or more steps in either direction can be traversed at the moment of mutation. But if chemical alterations of an organic molecule are involved, it is possible to have also a stellate pattern of relationships between the different mutants, in the manner shown in Fig. 1. As an illustration coming from outside the field of genetics, we may cite the stellate manner of interrelation of the substances that cluster about methyl glyoxal in the metabolism of carbohydrate. Some of the alterations are reversible in this case, and others irreversible (Fig. 2). A comparable stellate system of relationships is shown in the cluster of eye-color mutations in *Drosophila*, all occurring at the locus of a single gene. The diagram given here is

combined and adapted from several partial diagrams published by Timoféeff-Ressovsky (1930, 1932, etc.). All mutations that have been observed at this locus have been either to or from the three forms "normal", "eosine", or "white", as here diagrammed (Fig. 3). The arrows indicate the mutations, and the direction of mutations, that have been actually observed. Downward arrows represent mutations to a paler eye-color, and upward arrows stand for changes to a more strongly colored form. Statistics on the up-grade mutations are very inadequate, and probably incomplete because of their rarity, less than 9 per cent of the X-ray induced mutations being of this category. It is noteworthy that thus far "eosine" is the only form by way of which a reversion to "normal" has been observed to occur. Obviously the interrelations here shown do not represent a ladder scale, but a sort of triple stellate pattern in which "normal", "eosine" and "white" occupy positions of relative stability which serve also as centers of radiation. This pattern, and also the fact that particular transformations have occurred repeatedly, make very strongly for the hypothesis that a distinctive chemical genetic entity is being transformed into one and another of its nearest related substances. The argument is further fortified when we consider the multiple effects of these genes. In their effect on eye-color we have "white" < "eosine" < "blood" < "normal", but in reference to the color of the kidney of the mutant the first two genes give complete colorlessness, so that "white" = "eosine" < "blood" < "normal". In the

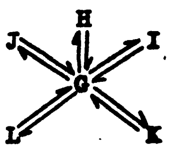

Fig. 1

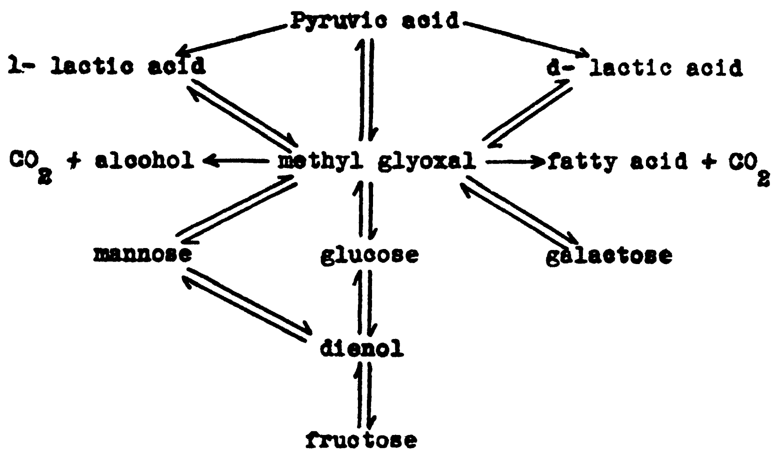

Fig. 2

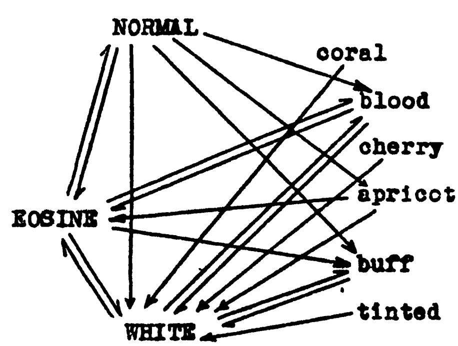

Fig. 3

degree of fertility shown in the different classes, "eosine" < "blood" < "white" < "normal". Finally in the vitality of the individual, "white" < "blood" < "eosine" < "normal". Thus the apparent serial arrangement of the same cluster of genes becomes radically different whenever we change the criteria of reference,—a situation better compatible with a qualitative than with a quantitative interpretation of these mutations.

It lies in the background of all these considerations of qualitative versus quantitative differences, that so far as we can tell, the qualitative differences of molecular constitution are everywhere the fundamental differences between species, and that differences of morphology, proportion, or even size as between species are with but rare exceptions, derived from the chemical differences and secondary to them. The morphological differences between the eggs of closely related species, if they are existent at all, are frequently beyond detection except by the most refined observation; yet it appears that serologically we must unfailingly anticipate a qualitative difference between their proteins. And as the organs develop, these immunological specificities of the various organ proteins remain prevalent at every stage, so that there is scarcely a protein in the body that will not react to show a qualitative difference from the corresponding protein of the next related species. In the face of such all-pervading chemical individuality, it becomes the natural inference that the gene differences, to which the somatic differences are due, are themselves also in large measure qualitative.

The quantitative hypothesis that mutations are largely alterations in the amount of active substance carried in a gene, demands obviously that we picture the genes as many-moleculed structures. Conversely the qualitative hypothesis opens the door to the concept of a one-molecule gene, a thought which has been touched upon by various authors (Morgan, 1928; Koltzoff, 1928), but most explicitly brought forward by Demerec. (Demerec, 1933, 1935; Timoféeff-Ressovsky *et al.*, 1935b.) A body consisting of a single molecule, or of a very limited definite number of molecules can, it is pointed out, be most easily imagined to go through these discontinuous, reversible, qualitative alterations. As a future test for this hypothesis Demerec has suggested that the white-eye genes derived respectively from blood, apricot and buff in *Drosophila* ought to carry a hidden difference, because they should still have in them the molecular peculiarities of the diluted gene by way of which each of them became white, so that a reverse mutation from white to color ought by statistical preference to go back to the same particular dilute color.

PHYSIOLOGICAL INTERPRETATION OF DOMINANCE

The introduction of a speed factor, as indicated earlier in this paper, leads toward a modified interpretation of Mendelian alternative inheritance, which fits advantageously into the physiological and biochemical picture of the organism. It can best be developed by reference to some of Goldschmidt's investigations (1927) of the gypsy moth, *Lymantria*. Geographical races of gypsy moths carry Mendelian genes for dark coloration versus extensive light spotting of the caterpillar. In the dark races the pale spots show in newly hatched larvae, but have grown dark by the time of the first molt. The pale races darken only under special experimental conditions, and even then only shortly before pupating. Homozygous intermediate races occur that darken at various

rates during the larval history. Heterozygotes of a light and a dark race simulate the intermediate races, being light enough after the first molt to give the impression that light dominates, yet dark enough after the last larval molt to permit the casual inference that dark dominates. (Goldschmidt, *op. cit.* p. 51–58.) A sound interpretation is only possible in terms of a progressive reaction in which elapsed time and either genic mass or catalytic potency of the genes are influential.

Even more notable are the experiments on intersexes among the species of these gypsy moths. The male-determining and female-determining factors—presumably genes—of this genus differ in a strength among the various races and species. Through hybridation it has been found possible to pit against each other sex chromosomes and autosomes so badly balanced in potency that moths which should be male, by tally of chromosomes, are somatically female, or, in other crosses, vice versa. Less extreme cases give inter-sex mosaics instead of complete reversal, and an analysis of these cases shows that the moths starting with this proper sex, proceed normally to a certain date in their development when the unduly potent genes representing the opposite sex begin to overstep the threshold for controlling the development. Everything produced later than this physiological turning point constitutes the reversed sex part of the mosaic. Each detail is in these cases true to one sex or the other and not a blur of the two. These experiments agree with those on the dark and light colored caterpillars in requiring a temporal progression under the influence of varying masses or potencies of the genes and of the enzymes (oxidases, etc.) whose rate of production they determine. The occurrence of a definite point of time at which the threshold is crossed for a sort of reversal of dominance is extremely instructive, and cannot fail to influence our general concepts of alternative inheritance.

Goldschmidt's hypothesis (1927) that the effect is referable to the speed differences elicited by different masses of competing genes seems entirely acceptable as an hypothesis, and open only to the reservation that different levels of catalytic effectiveness can equally well spring from minor qualitative differences in the molecular structure of the catalyst or gene. The belief shared by Morgan, Goldschmidt, and other authors that recessiveness need not imply the absence of a gene is strongly supported by these intersexes.

GENES AND "ENTWICKLUNGSMECHANIK"

The question how the genes do their work in the organism is anything but a simple one, and any attempt to report its present status would involve an encyclopedic synopsis of the field of Entwicklungsmechanik such as cannot be ventured upon in this paper (Schleip, 1927; Ekman, 1930). Yet at least a few cursory words on the subject are needed in the present connection.

We note first of all that the same gene will not exert the same effect at all times and at all places. The minor but indubitable differences that occur in some of the cases of gene translocation may serve as an illustration (Dubinin and Sidoroff, 1934). This seems to be explained by the consequences of placing genes where they come under the influence of a changed assortment of gene neighbors.

It is obvious that no matter how particularistic the mechanism of heredity may be, the living organism, whether embryo or imago, is no mere assemblage of listable genetic traits, and the fertile germ cell contains no pattern, either actual or symbolic, of the organism that it blossoms into. The gene theory means, rather, that the germ cell nucleus is provided with a numerous but definite assortment of material particles having a

regulatory function, and that these particles initiate the crucial happenings that become determinative of the character and the course of progression of the cellular activities, and thereby eventually of the morphology, physiological make-up and functioning of the developed organism. Every cell in the body carries a complete set, or reasonably near complete, and presumably the whole set is in the main functional in every cell at least as long as the cell is developing, and in all probability as long as it has a functioning nucleus. Each unit of the genic complex stands in active, intimate relationship to other units present in the same nucleus, to cytoplasm, and to more remote physical and chemical influences, all of which items, both intracellular and extracellular, are in a sense environmental to it. In the different tissues all factors lying outside the nucleus progressively become highly divergent, as the individual develops. Thus there must come to pass a perpetual shifting of the relative effectiveness, and even of the qualitative effect of the action emanating from any one gene, due to the changing medium in which it occurs. In one ontogenetic environment the climax effect of a particular gene may come early, in another it may come later, thereby inducing a sharply different end result (J. B. S. Haldane, 1932). The progressive specialization of the cytoplasm to which we have just alluded is described in different terms by Just (1936).

This author puts greater emphasis on the cytoplasm as a prime seat of the manifold vital processes. He points out that the nucleus stands in a sort of parasitic relation to the cytoplasm, from which it obtains its metabolic wherewithal. In doing so, he argues, it may easily strip the cytoplasm of the material anlage for one and another physiological potentiality, thus bringing about a specialization of the cells. The genes are accordingly interpreted as regulators of the stripping (and specializing) process, while the cytoplasm is described as progressing—or degenerating—from a state of multiple potentialities and abundant endowment to a state of one-sided specialization. Although this was offered as a substitute theory, there does not seem to be any *prima facie* incompatibility between the mechanism it suggests and the more orthodox picture of gene action. Conceivably one gene might work in both ways. Just's hypothesis has the merit of agreeing best with the usual interpretation of the Entwicklungsmechanik of tissue differentiation.

In the processes of ontogeny it is constantly observed that the different parts of an organism not merely keep step with each other chronologically, but are actually influenced by each other so that their various differentiations are actively held in step even under somewhat abnormal experimental conditions. This "integrative" effect freely oversteps the cell boundaries and necessitates the handling of many embryological problems from the standpoints of tissue masses, chemodifferentiation (Julian Huxley, 1932), organizer substances (Spemann, 1927; Needham, 1931 and Needham *et al.*, 1934), metabolic gradients (Child, 1915), etc., in place of cell lineage and cellular mosaic. In cases of regeneration, which we may look upon as a sort of belated embryology, cell masses frequently develop in directions totally contrary to the implications of their cell lineage. At first glance these findings seem at variance with any particularistic theory of heredity, but the answer has been made that the gene theory differs from all other particularistic theories of the past in such a way as to give it an escape from this criticism (Goldschmidt, 1927). This is partly because it ascribes in the main similar sets of genes to all cells of the developing organism, making it conceivable for cells with dissimilar embryonic history to replace each other, and partly because it must explain the specialization of organs and tissues as due to the accumulation of differential physiological conditions exterior to the

genes, viz. to the above-mentioned chemo-differentiation, hormone influences, metabolic gradients, etc. Its own findings and postulates force it, that is, to become what numerous authors have protested was necessary in any good theory of heredity—physiological in viewpoint.

THE GENE PICTURE IN MAN

The study of mammalian and human heredity involves a number of difficulties, not the least of which is the slow rate at which data can be accumulated. It would take several thousand years to assemble in man such experimental pedigrees as have already been obtained for fruit-flies. Hence, unless we would leave the discussion of human heredity to our remote successors in science, we shall have to proceed to a considerable extent in terms of analogy and comparison.

Our count of chromosomes has been reported variously from different laboratories.

The haploid number, found in female-producing sperms and in unfertilized ova is reported to be either 24 or 12 chromosomes. This probably represents a difference in the state of aggregation rather than a direct total discrepancy in the biological material, since the universal inter-varietal fertility of our species is evidence of a fairly constant total count and arrangement of genes in man. The male-producing sperms are credited by different authors with either 23, 24, or 12 chromosomes, the chief disagreement here being as to the presence or absence of a small Y-chromosome. (Wieman, 1917; Winiwarter, 1920–21; Painter, 1923, 1924.) The preponderant evidence is that the Y exists, but functions genetically as very nearly a blank. (Oguma and Kihara, 1923.) The male diploid count of effective chromosomes is accordingly 47, 48, or 24, as against a female diploid count of 48 or 24. (Schachow, 1926.)

Data have been accumulated sufficient to show that the Mendelian rules apply in principle to long lists of human traits. Frequently, however, the same measurable trait is under the influence of genes in different chromosomes, with a complicating effect. The details of these extensive studies hardly belong in this paper, although they are eugenically extremely important, and serve to demonstrate the likeness between heredity in man and in other species. It is of especial interest that in addition to the usual bodily characteristics various neurological peculiarities and even certain traits of the mental constitution have been reported as heritable in the Mendelian manner.

There is thus far only a very limited list of gene linkages known in man. This is because a zygote count of 47 or 48 chromosomes means that only by special luck or industrious search will observers obtain statistics on two genetic characters carried in the same chromosome in the same human pedigree. The best existing series of this sort was collected by Bell and Haldane (1936) for two X-chromosome characters, hemophilia and color-blindness. (Cf. also Hogben and Pollack, 1935; Yorshis and Gottlieb, 1934; Shōji and Ninoyu, 1935; and Zieve *et al.*, 1936.) They find this linkage to be very pronounced, 17 test cases giving 16 examples of linkage and one doubtful example of cross-over.

To the genealogically inclined it will be bad news that no person is effectively descended from all his reputed ancestors. Our 47 or 48 chromosomes come 24 from the mother, the rest from the father; but from grandparents and earlier ancestors the derivation follows the mathematical rules of "pure chance." Discounting Y as a "blank", a paternal grandparent of a boy has one chance in 8,388,608 (i.e. $1:2^{23}$) of failing to contribute a significant chromosome to the grandson. A maternal grandparent has correspondingly one such chance in 16,777,216 (i.e. $1:2^{24}$). A girl's paternal grandmother is sure to have provided her with an X-chromosome, and has one chance in 8,388,608 that this is

the only one. The chances for other grandparents are the same as in the case of the boy.

There is a 46 per cent likelihood that a particular maternal grandparent supplies between 11 and 13 of the child's chromosomes (inclusive); a 69 per cent likelihood that the share runs between 10 and 14; and less than one chance in 300 that his share is 5 chromosomes or less.

The statistical case for great-grandparents is far less favorable. If we disregard the problem of "cross-over", we find there is 1 chance in 996 that in a particular infant of unidentified sex the great-grandparent in question is not represented by any chromosome. Adding one more generation, and there are better than even chances (509:491) that not more than 15 of the great-great-grandparents have any of their chromosomes present in a specified descendant of unidentified sex. The phenomenon of cross-over will modify all these figures on the distribution of genetic material to a degree not as yet determinable, but always in the direction of wider representation, and ever-increasingly as the generations progress. The maximum limit of effective ancestors is our quota of genes, whatever that may be. In no case will the figure be high enough to provide more than a gambler's chance that an ancestor who, let us say, brought fame to the family at the battle of Crécy, has actually contributed a single gene to a particular descendant living today for there cannot possibly be enough genes to share around among the half million ancestors of that date.

An especial contribution from the human realm is the comparative study of "identical" twins, that is to say, of two individuals originating from a single fertilized ovum, and hence provided with identical assortments of genes (Rife, 1933). From such data we should learn just what limit there is to the precision of hereditary control. On the physical or ordinary somatic side a remarkably close correspondence has been recorded between twins of this type. For example it is demonstrated that in such minor items as the fingerprint patterns the genes are in some way responsible for the pattern type, a matter in which ordinary brothers and sisters show great diversity, but that the actual count of the epidermal ridges by which the pattern is executed is not closely controlled by genes. (MacArthur and MacArthur, 1937.) In most cases the similarity of pattern between corresponding hands of identical twins is about comparable to that found between the two hands of one ordinary individual.

When we turn to mental development and social characteristics the evidence becomes blurred by uncontrollable external complications. The question is here twofold; (1) How similar are individuals in which both the genes and the environment are alike? and (2) How similar will individuals remain if their identical hereditary traits are subjected to contrasted environments? The answer to the second question shows that although the individuals become different, the degree of difference is appreciably less than is statistically expected for ordinary brothers with contrasted environments. For the first question, however, a clear answer is almost out of the range of possibility, because identical environments cannot exist for two human individuals, even in the same family surroundings. It is impossible even for twins to remain in reciprocally identical social relations to each other. For example in the matter of leadership in their mutual relations, either one will become habitually the leader, or their leadership will become distributed between them in a topical manner, eventually hardening into fixed

habits and traits of character. Their own desires to be alike, or to be unlike, will also skew the results. Hence there is need for very discriminating statistical study on a larger scale than has yet been attainable.

GENES IN INTER-SPECIES STERILITY AND DIVERGENT EVOLUTION

During the evolutionary discussions of the 19th century it was pointed out by J. T. Gulick (1872, 1887, see also 1905; and Addison Gulick, 1932) by Romanes (1886, 1897) and by Karl Semper (1881) that inter-species sterility constitutes one of the major conditions for the progressive evolutionary divergence of species from each other. Semper commented (edition of 1889) that geographical isolation, which plays such a prominent rôle among land and coastal species, is virtually non-existent in the whole realm of pelagic plants and animals, so that here the entire process of species formation is conditional upon physiological isolation. More recent authors have called renewed attention to the evolutionary significance of sterility between groups (Sewall Wright, 1931; Robson, 1928; Robson and Richards, 1936.)

Genetics and serum biochemistry suggest several possible roads by which a condition of inter-species sterility may come to pass:

(1) The translocation of groups of genes into chromosomes not homologous to the ones originally occupied. Where one such accident of translocation has occurred, offspring are produced, only ½ of whose germ cells are viable. With multiple translocations it has been found that only $(\frac{1}{2})^n$ of the germ cells are viable, n being the number of translocations. If n becomes sufficiently large this will virtually establish an inter-species sterile-mule condition, the state to which J. T. Gulick gave the name "Segregate fecundity." Its distinctive characteristic is that the fertilization of the ovum is accomplished, but adequately fertile off-spring do not result.

(2) Closely related to the last, if there are extensive inversions and other rearrangements of the gene chains even without their transfer to other chromosomes, there results an accumulated embarrassment of the process of synapsis in hybrids, that may contribute toward establishing a sterile-mule condition.

(3) Another form of segregate fecundity has been reported, which is dissimilar for reciprocal crosses, A × B being fertile, yet B × A producing only sterile results, at least for one of the sexes. This is the situation where translocation of essential genes has occurred between the X- and the Y-chromosomes.

(4) Theoretically a situation should be found where a gene mutation sets up a new serological condition that makes the genital tract fluids or the germ-cell sap incompatible with those of the old stock, thus preventing any fertilization from occurring. Conditions more or less of this sort are thought to occur in nature, and were named "Cross impotence" by J. T. Gulick. This differs from segregate fecundity in that the ovum is untouched, and hence is not prevented from becoming fertilized by a more appropriate male. No mutation of this sort has been observed occurring in the laboratory, and the genetic background for this supposed condition is still unknown. It is not necessary to assume that such a serological change need depend on the mutation of any great number of genes, especially as we note that the mutations whereby certain human bloods become agglutinative of other bloods are referable to the alteration of one or another among two or three genes. In view of the manifold qualitative differences of protein constitution detected even in closely related species, evidence for inter-sterility of this sort as a result of gene mutations ought to be discoverable.

(5) Another angle of this problem may be seen in what are known as fertility and viability genes, and especially the possibility that there are genes that may function as conditional lethals. Each of these categories admits of situations that may increase the sterility between dissimilarly constituted germ plasms. The conditional lethal effect may be illustrated from the fruit-fly mutations known as eyeless, bent-wing, and shaven. These all occur in the small chromosome, and all produce viable results both in the heterozygous and homozygous conditions. If, however, a fly happens to be haploid for the small chromosome, the presence of any of these recessive genes becomes lethal. It is but a short extension of this to suppose that one species may have genes that are lethal unless offset by certain other genes of the same species, or liable to set up lethal processes if inappropriately combined with certain alien genes. Many facts of interspecies sterility would fit into

some such genetic pattern, but it is evident that in a field where so many alternative hypotheses can be suggested the real need is for fuller experimental data.

Dobzhansky cites cases in the genus *Drosophila* which he interprets as belonging to this category of conditional lethals, although they give a first impression of being in category 3 on account of a preponderant lethal effect in the male sex. (Dobzhansky and Boche, 1933; Dobzhansky, 1937.)

Thus with the progress of years we find a striking reinforcement of the scientific cogency of the theory which Romanes and J. T. Gulick championed; namely that a physiological barrier between two otherwise hardly distinguishable stocks may occur frequently, and must have the effect of initiating a train of divergent evolution. But today it would be restated in the modern wording that it is possible for two forms very nearly alike in their total assortment of genes to be mutually sterile by virtue of one or two trifling mutations, or even by relocation of genes without any alteration of total content, with a result that most initiate a divergent evolution.

OTHER EVOLUTIONARY IMPLICATIONS

The gene theory, with its picture of a not unlimited quota of genes, each of which is subject to such mutations only as are permitted by its status as an organic chemical molecule (or small aggregate of molecules) gives us a very different mental image of what happens when a species undergoes alteration than would be gathered from any of the earlier theories of heredity. We note:

1. Since the primary origin of gene mutations is from physicochemical causes, and their actual nature must be that of a chemically definable alteration of the gene substance, we find that from the biological viewpoint these mutations have a decidedly unteleological and even inconsequential flavor. Genetic adaptation apparently has to do with what the evolutionary forces can accomplish out of these mutations, rather than with the primary nature of the newly produced genes.

2. Mutation possibilities are restricted to such changes as can occur and be perpetuated in the chemical configuration or numerical readjustment of genes already present. From this it follows that:

(a) The *directions* of possible mutations are not perfectly *ad libitum*, but are limited to changes in such directions as can find expression in the gene complex actually on hand.

(b) Variation in any one direction cannot be subdivided into infinitesimal gradations. Chemical changes in physical particles that approach molecular dimensions are necessarily discontinuous.

(c) The total ultimate *extent* of variation in any one direction is theoretically subject to the limitations of what can be expressed in a finite number of genes, each with finite variability. This principle should eventually become important to students of evolution, although it would seem audacious to invoke it at present in connection with any specified problem in evolution.

3. The experimental fact of linkage covers two types of phenomena, viz. effects referable to different genes located in the same chromosome, and those referable to a single gene at work in different tissues of the body. The former has only a transient significance, because of the possibility of cross-over; but the multiple effects, perhaps seemingly unrelated, which come from the several influences of one gene, are a real limitation upon the path of evolution. Certain color genes are, for example, impossible to fix, because in addition to their color effects they function as recessive lethals. Considerations of this sort may eventually supply explanations for many otherwise unaccountable vagaries of evolution.

4. Despite the finite number of genes and of gene mutations, the factorial system of inheritance provides possibilities for an incredibly copious diversity of individual heredity, which serves as the crude material upon which natural selection can work (Sewall Wright, 1931). On the principle that each new mutation doubles the number of theoretically available mutation combinations, the known mutations in *Drosophila*, more than 400 in number, should permit of 2^{400} (= to about 10^{120}) different conceivable combinations. The unimaginable magnitude of this figure is illustrated by the fact that the total mass of the astronomically known universe expressed in milligrams has only been estimated to reach the figure $10.^{60}$

5. In the light of the gene theory biparental propagation is found to be much more conducive to individual diversity than might have been supposed, and this effect is much less impaired by temporary inbreeding than other theories would lead one to anticipate. The chances are infinitely against identity of the genes carried by two brothers in a family derived from an ordinary population. Formulae developed by Sewall Wright (1922, 1923) indicate that starting with outbred stock and disregarding linkage, with its complications, it requires three generations of brother-sister breeding to accomplish statistically a 50 per cent diminution of heterozygousness among the genes retained, and that ten such generations will only diminish the heterozygousness by 89 per cent. Such persistency of the heterozygous condition has never before been suspected, especially as it has long since been recognized as a commonplace that 10 generations of hermaphroditic self-fertilization (as in plants) will give not less than a 99.9 per cent elimination of heterozygousness.

6. The stability and persistence of genes saves a character from being entirely lost through outbreeding. Even a recessive may live on unsuspected, only to reappear when a mating, perhaps between remote kin, brings it to light again. This principle can do for evolution some of the things for which isolation had to be invoked in the pre-Mendelian days. (Robson, 1928; Robson and Richards, 1936.)

7. The stability of genes also augments the effect producible by isolation, since it provides an accumulation of hereditary potentialities which may be brought to light by isolation and its resulting inbreeding.

8. Any mutation that can occur once can occur again, in the sense that if one molecule of substance *A* has transmuted into substance *B* we may well anticipate that other molecules of *A* will undergo the same transmutation. This contrasts with the old viewpoint, according to which small variations were indefinitely intergraded in all directions, and larger "sports" were too much in the nature of freaks to permit the likelihood of their repetition. It may be inferred that even without the inbreeding of relatives, a new homozogous character may become established through the mating of diverse stocks in both of which the new gene has independently originated.

9. The significance and effectiveness of natural selection can now be tested by appropriately ordered experiments, and appraised by pertinent mathematical analysis. This possibility had to wait upon the acquisition of reasonably correct formulae for the mode of action of heredity and mutation. (Fisher, 1930; Sewall Wright, 1931, 1934, 1937; Haldane, 1936.)

(To be concluded)

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THE SEX RATIO IN MULES AND OTHER HYBRID MAMMALS

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INTRODUCTION

A SURVEY of the literature discloses that the relative proportion of the sexes in mammals began to receive the attention of observers about the middle of the 18th century, and that the sexes among hybrids were the first to be recorded, although the numbers were very small. The meagerness of such records is attested by Darwin (1874), who stated, that so far as known to him no one had paid attention to the relative numbers of the sexes throughout the animal kingdom. Apparently this statement by Darwin aroused the interest of investigators, since numerous observations on the proportion of the sexes were recorded for various species of mammals and birds between 1880 and 1910 and much discussion entered the literature concerning sex ratios and the determination of sex.

Buffon (1791) was one of the first to call attention to the proportion of the sexes in hybrids. He expressed the opinion that an excess of males occurred. Colonel Hamilton Smith (1841) stated, that it is observed in general that males are much more abundant than females in hybrids and that the fact is equally true in mules, where the males occur in the proportion of two or three to one female. Gadeau (1899) also stated, that the results of hybridization are much oftener males than females, and that male offspring are more numerous in proportion to the specific distance between the two parents. Suchetet (1897) expressed the opinion that in hybrid birds males are more numerous than females and cited numerous observers in support of this opinion.

In the early discussions pertaining to the sexes of hybrids the writers frequently neglected to distinguish between mammals and birds. This, together with the fact that males do occur more numerously than females among hybrid birds, may account in part for the view that males are in excess in hybrid mammals as well.

Haldane (1922) gave a discussion of the sex ratio in hybrids based only on cases bred in captivity and for which more than 10 offspring were raised. Furthermore he included only cases where one sex was absent, sterile or the sex ratio was markedly disturbed. From this study he advanced the hypothesis that when in the F_1 offspring of a cross between two animal species or races, one sex is absent, rare or sterile, it is the heterogametic sex.

Because of the need for a more extensive investigation than has heretofore been made of the proportion of the sexes in hybrid mammals the present study was initiated. The attempt has been made to assemble as many published records of the sexes of hybrid mammals as are available, and to collect as large a number as possible of actual birth records of the sexes for one or more interspecific hybrids. Furthermore, it is desired to make a comparison

of the sex ratios found for hybrids with the normal sex ratio for the species wherever possible, in order to determine with reasonable precision how much hybridization actually influences the proportion of the sexes. Since the mule is so commonly produced for commercial purposes this hybrid appeared to offer the best possibility for securing reasonably large numbers.

At the outset it was hoped that the desired records might be obtained from farms and ranches which had been producing large numbers of mules. However, a survey of the mule-producing territory in the United States revealed that records of the sexes had not been kept by those engaged in mule production. Furthermore, that ranches that had formerly made a business of producing mules had largely discontinued the practice. Jacks are now owned by individuals and the mules are produced by farmers who breed one or more mares yearly to a neighboring jack stood for this purpose. Since farmers seldom make records of the sexes of colts and produce only a small number of mules individually, some plan had to be followed which would yield foaling records from a large number of farms. A plan for obtaining records through the cooperation of farmers and jack owners was outlined and presented to the Committee on Animal Breeding for the National Research Council for suggestions. This committee approved the plan and urged that the investigation be made. Acknowledgment is made for suggestions and encouragement received from the committee.

The study was begun at the University of Wisconsin, and continued at Oklahoma Agricultural and Mechanical College. Doctor L. J. Cole proposed the problem and has given helpful suggestions and assistance in obtaining the data and preparing the manuscript. Professor W. L. Blizzard has made it possible to continue the study. Grateful acknowledgment is made to jack owners, farmers, county agents, vocational agricultural teachers and animal husbandrymen who have cooperated in obtaining the records.

RESULTS

1. Sex ratio of the horse

Darwin (1874) was apparently the first to report the proportion of the sexes for the horse. He obtained 25,560 records from the "Racing Calendar" for the thoroughbred and found a ratio of 99.7 males to 100 females. Wilckens (1886) and Düsing (1887, 1888) gave reports based on records secured in Germany from government horse breeding farms and private records, which had been recorded for a number of years. Wilckens' material included more than 16,000 records and shows a sex ratio of 97.3. Düsings' earlier report contained more than 800,000 records with a sex ratio of 98.31. His later report includes more than a million records and gives a sex ratio of 98.75 or 49.69 ± 0.002 per cent males. The earlier ratio given by Düsing appears to be quoted more often than any of the others in discussions of sex ratios.

Kisslowsky (1932) points out that Cornevin (1890) found the sex ratio of the horse to be 101, and further that Baldassare (1896) reported it to be 102. He also cites von Oettingen (1921), who maintains that more male than female horse foals are born, averaging approximately 106 males to 100 females. Calder (1927) is cited by Crew and Smith (1930) for his study of the sex ratio of the horse, based on the Clydesdale stud-book of England. He found the sex ratio to be equality.

Since the earlier records gave an excess

of females and later records showed an equality of the sexes or a slight excess of males, some doubt appears to be justified concerning the dependability of the sex ratios given. It is difficult to know how complete the records were. McPhee (1927) has shown that swine herd-book records are not sufficiently accurate for the establishment of reliable sex ratios. There is no apparent reason to indicate any greater degree of reliability of stud-book records for this purpose. Therefore, a need for complete foaling records was realized in order to make a comparison with the sex ratio obtained for the mule.

In attempting to find foaling records contact was made with the Colleges of Agriculture in the United States and records were obtained through the cooperation of eleven institutions and one horse breeding farm, as follows: Univ. Ill.; Ia. Sta. Coll.; Kans. Sta. Coll.; Univ. Minn.; Univ. Mo.; Cornell Univ.; Ohio Sta. Univ.; Okla. A. M. Coll.; Tex. A. M. Coll.; Univ. Wis.; Univ. Wyo.; Prairie Farms, Alicia, Mich. (courtesy J. F. Ziegler, Mgr.). Grateful acknowledgment is given for these records furnished by those in charge at the different institutions. A summary of the records shows; live-born foals (single births) 626 males and 581 females; still-born 20 males and 8 females; sets of twins, 3 pairs of males, 4 pairs of male and female, and 7 pairs for which sex was not recorded. The total sexes recorded are accordingly 656 males to 593 females, the sex ratio being 110.6 or 52.52 $\pm$ 0.95 per cent males. The sex ratio of the live-born foals is slightly lower, being 107.7 or 51.8 per cent males. At this point it should be said that the probable error, rather than the standard error, has been used throughout this paper.

Efforts to obtain information as to the normal sex ratio of the ass have been unsuccessful.

2. *Sex ratio of hybrid Equidae*

a. Hybrids previously reported

In Table 1 a summary is given of the sexes of the hybrid Equidae which were found recorded. Although the numbers are small females are in excess, giving a sex ratio of 44.74 $\pm$ 5.45 per cent males. Ewart (1899) reported 9 hybrid foals by a zebra stallion mated to mares, but sex was given for only 8. In a later discussion (1910a) he called attention to 16 hybrids of this cross but the sexes of these were not mentioned. Pocock (1911) described a hybrid between a wild ass and a quagga mare but the sex was not recorded.

In response to our inquiry for information concerning the sexes of hinnies (progeny from female ass, or jennet, mated to stallion), Mr. R. Lynch, of the Department of Agriculture, Dublin, Ireland (In litt. 1932), states:

> Regarding the sex ratio among Hinnies it would appear from the reports furnished by our local officers after examination of upwards of 1,000 animals that females predominate. The percentage works out at 58% females and 42% males. On the other hand in mules the number of males appears in excess of females, the proportion being 61% males and 39% females.

Statistics on the sexes of mules in Spain for 1928 were secured from the American Consular Service (In litt. 1932, courtesy Mr. Curtis C. Jordan). According to the information obtained there were 458,542 males and 410,106 females, a sex ratio of 52.79 per cent males. Since these reports do not represent foaling records, and may therefore be selective, they are not considered to be sufficiently reliable for use.

According to Lotsy (1922), Gottschling reported 196 matings of female asses to a stallion, from which only 10 colts were born. From 25 matings of female asses

to a zebra stallion 3 foals were born. In 119 matings of mares to jacks 51 became pregnant. The sexes of these hybrids were not given but it was mentioned that all of them were infertile. Tegetmeier and Sutherland (1895) stated, that 3 zebra hinnies from a female zebra and by small stallions were seen in Sir Henry Meux Park, but again sex is not given.

b. The mule

(1) Plan and source of data

The plan followed in securing the data herein reported for the mule was to make contact with reliable persons who were sufficiently interested in such a study to assist in getting the records. County agricultural agents and vocational agricultural teachers were approached on personal acquaintance or on recommendation by animal husbandrymen, and the coöperation of several was enlisted. Blank forms were prepared and placed in the hands of those who had agreed to assist; they distributed the blanks to reliable jack owners, who supplied the data from foaling records kept for the service of their jacks. In a few cases the coöperators secured service records for one or more jacks and followed up each mare and obtained a report of the sexes from the mare owners.

The plan was followed for two years. By this time a number of jack owners had become sufficiently interested to volunteer their services in supplying records directly, and blanks were sent to them, which they returned at the end of the foaling season. Complete records were obtained in all but a few cases. The few incomplete reports were due to the sale of mares by the owner following service, so that contact was broken; also a few mares aborted while in pasture and the sex of the foal was not observed. An appeal was also made to the Agricultural Colleges for records of the sexes of mule foals but only two had such records.

TABLE 1

Summary of the sexes of hybrid Equidae (exclusive of the mule) reported by various authors

MALE	FEMALE	M	F	AUTHORITY
Zebra	× Mare	3	5	Ewart (1899)
Zebra	× Ass	6	5	Riley (1909)
Zebra	× Mare	3	5	Roberts (1929)
Zebra	× Mare	1	0	Sclater (1903)
Zebra	× Mare	1	0	Hesse (1899)
Ass	× Zebra	0	1	Pocock (1911)
Quagga	× Mare	0	1	Ewart (1910) Lord Morton Case
Horse	× Ass	2	2	Anonymous (1890)
Horse	× Zebra	1	0	Iwanoff (1905)
Shetland	× Bergzebra	0	1	Dr. Lutz Heck (*In litt.* 1932)
Riesenesel	× Chap. Zebra	0	1	Dr. Lutz Heck (*In litt.* 1932)
Przewalskii (wild)	× Zebra	1	0	Gunali (1933)
		18	21	

(2) Sex ratio of the mule

The sex records collected for mule foals by the plan described, including the foaling seasons of 1929 to 1932, are shown in Table 2. For the 1416 mules (627 males and 789 females) sired by 98 jacks the sex ratio is 44.28 ± 0.89 per cent males (79.5 males: 100 females). This is practically the same ratio as obtained for all other

equine hybrids as summarized in Table I. Records for an additional 138 mules (74 males and 64 females) were obtained from the Mississippi Agricultural Experiment Station through the courtesy of Professor H. H. Leveck. However, the sexes for these were not recorded until the mules were one or more years old. The number of each sex that died or was sold before the records were taken on this group is not known. Therefore, these records are excluded from the table.

(3) Sex ratio of mules by different jacks

The percentage of males sired by each jack was calculated and the results for jacks that sired 3 or more foals are presented is Figure 1. The distribution shown in the figure indicates that these data are a random sample of jacks and that there is no evident tendency for certain jacks as compared with others to sire a preponderance of one sex. The fact that some jacks have produced foals all of one sex is satisfactorily accounted for on a basis of chance.

TABLE 2

Sexes of mules reported from different states

STATE	NO. OF JACKS	LIVE-BORN FOALS		STILL-BORN FOALS		SETS OF TWINS		
		M	F	M	F	mm	mf	ff
Ala. (1)	2	22	17	1	0	0	0	0
Iowa	5	6	9	0	1	0	1	0
Ill.	14	136	174	16	11	0	0	1*
Kans.	4	30	41	5	4	0	1	0
La. (2)	3	17	10	0	0	0	0	0
Mo. (3)	8	46	74	5	9	0	0	1
N. C. (4)	6	26	25	0	0	0	0	0
Okla.	52	269	341	6	6	0	2	4**
Tenn. (5)	4	37	49	1	2	0	0	0
Total	98	589	740	34	33	0	4	6

1. Through cooperation of Mr. Matthew Woods, Tuskegee, Ala.
2. La. Agr. Exp. Sta. courtesy Dr. Chas. I. Bray.
3. Through cooperation of Prof. E. A. Trowbridge.
4. Through cooperation Mr. Chas. A. Sheffield.
5. Through cooperation of Dr. Moses Jacob.

* Still-born twins.

** One set still-born.

The sex ratio found for the mule is 8.24 per cent lower than that found for the horse in this study. This is 6.3 times greater than the probable error of the difference. Even when the sex ratio for the horse reported by Düsing (1887) is used the sex ratio for the mule is 5.41 per cent lower than for the horse. This difference is 6 times greater than its probable error. Accordingly, in either case, the difference is distinctly significant.

(4) Fertility of jacks compared with that of stallions

Reports for the number of mares bred to 75 different jacks were complete for both breeding and foaling data. Only 51.9 per cent of these mares produced foals. This is 9.9 per cent greater than Gottschling observed according to Lotsy

(1922). He found that only 42 per cent of the mares bred to jacks became pregnant. thoroughbred were successful. Hammond (1914) mentions that the Royal

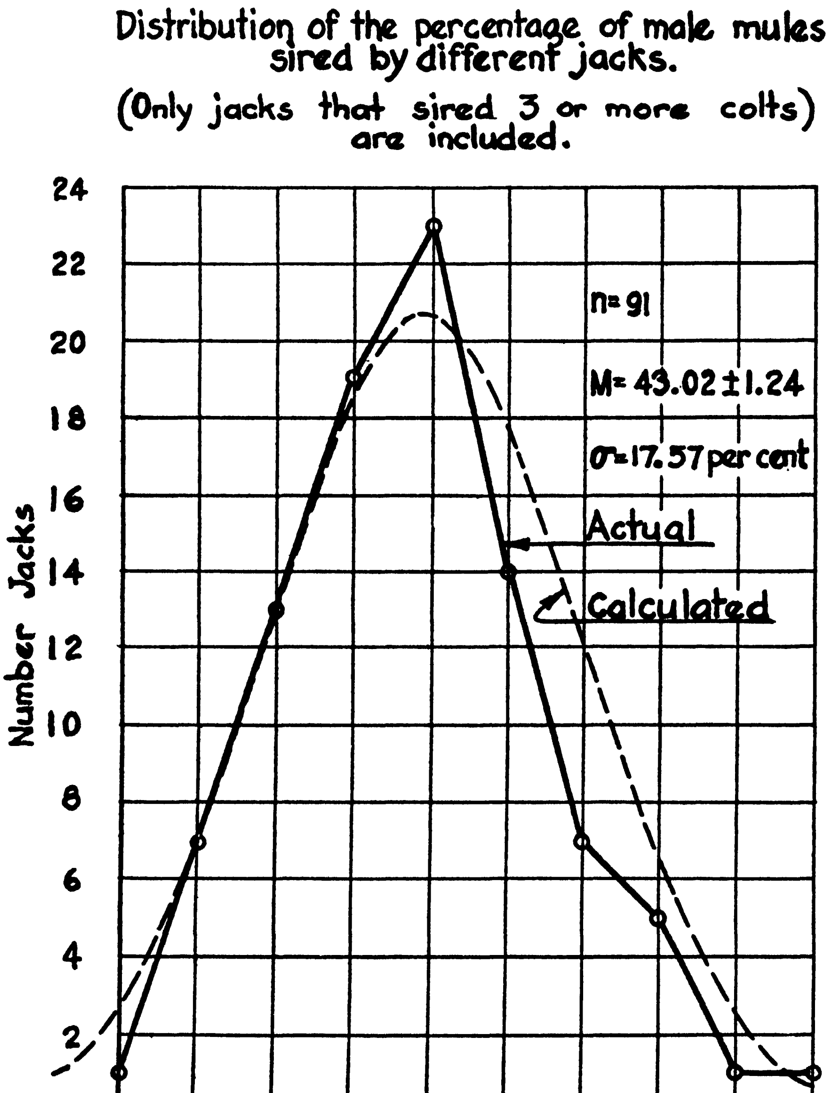

Fig. 1. Percentage of males.

Darwin (1874) mentions that data furnished to him by Tegetmeier from the "Racing Calendar" show that two-thirds (66 per cent) of the matings for the Commission on horse-breeding found that in mares about 40 per cent of those selected for breeding failed to produce foals. Kisslowsky (1932b), using selected stud-

book records for a group of 83 stallions and 83 of their sons (Cremer's data), found that 61.38 per cent of the matings for the fathers and 57.7 per cent of the matings for the sons resulted in conceptions. Sanders (1926) found the mean percentage of fertile matings for more than 1800 stallions to be 53.54 per cent. It is of interest, however, that one group of stallions gave a mean fertility of 57.37 per cent for the period 1887 to 1910, and for another group covering the period from 1911 to 1924 the mean fertility was only 51.53 per cent. A fairly constant decline in fertility occurred from approximately 61 per cent at first to 50 per cent at the end in Sanders' study. Gottschling (Lotsy, 1922) found that only 5.1 per cent of the matings of stallions to jennets were fertile, while 12 per cent of the matings of zebras and jennets were fertile. On a basis of these reports it appears that the percentage of fertile matings may be lower for jacks than for stallions when both are mated to mares. This might be accounted for in part by a lower sex ratio among mule than among horse colts. However, two other reasons appear as possibilities; (1) the advanced age of the mares bred to this group of jacks in this period, (2) mares are often put to a jack after they have repeatedly failed to conceive to the service of a stallion. Some of these mares are believed to be sterile.

(5) Still-births and twins

The records show (Table 2) that 5.01 per cent of the mules were still-born, and two pairs of female twins were among these. Although more females than males were still-born the difference is insignificant. Ten sets of twins were reported, which is a frequency of only 0.71 per cent of the total births. No case was reported for which the twins were both males. Sex was recorded for 40 still-born horse foals; 31 of these were males, a sex ratio of 77.5 per cent. There were 14 still-born horse foals (twins) for which sex was undetermined. Thus 54 horse foals were still-born. This is 4.28 per cent of the 1263 foals. It is also of interest that 64.3 per cent of the horse twins were still-born. The frequency of twin births is 1.12 per cent, which is exactly the frequency to be expected according to Richter (1926).

3. Sex ratio in the Bovidae

Johansson (1932a) has recently made an exhaustive study of the normal sex ratio from herd records in the Swedish and Finish breeds of cattle and found it to be 51.52 per cent males. This is in close agreement with the ratio commonly reported for cattle (*Bos taurus*).

Unpublished calving records of the sexes for 8646 head of *Bos indicus* were obtained through the courtesy of Colonel Matson, O.B.E., I.A. Civil Lines, Jubbulpore, India, and for 124 head from the Louisiana Agricultural Experiment Station (through courtesy of Dr. Chas. I. Bray). The sex ratio of the latter is 51.6 per cent males. Combining the two sets of records gives a total of 4466 males and 4304 females, or a ratio of 50.92 ± 0.36 per cent males. This is very close to the normal ratio for domestic cattle (*Bos taurus*).

a. Sex ratio of hybrids

The figures shown in Table 3 are a summary of the sexes of F_1 hybrids which have been found for the Bovidae. For the cross of bison and cattle 19 males and 62 females are shown, a sex ratio of 23.5 ± 3.18 per cent males. It is frequently mentioned by those who have reported this cross that much difficulty arises due to the presence of excessive amniotic fluid in the female cow carrying the hybrid calf. Furthermore, that the hump on the male calf increases calving difficulties for *Bos*

taurus females. Boyd (1908, 1914) reported 6 males and 39 females from crossing American bison bulls on domestic cows. Three of the males died at birth and one lived only a day. The two surviving males proved to be sterile. Fifteen of the females failed to produce calves although mated to both bison and domestic bulls. Only 3 female cattalo, as the hybrids are called, were reliable breeders. Hybrid females mated to bison bulls produced 12 males and 18 females. Boyd also points out that 63 cows mated to bison bulls aborted, but he does not report the sexes of the calves. Deakin, Muir and Smith (1935) reported 11 abortions and 10 still births for which the sexes of the calves are not given. However, the two males shown in Table 3 by these authors are reported as live-born.

TABLE 3
Summary of the sexes of hybrid Bovidae

MALE	FEMALE	M.	F.	AUTHORITY
Amer. bison	× Dom. cow	6	39	Boyd (1908, 1914)
Amer. bison	× Dom. cow	1	3	Kühn (From Lotsy 1922)
Amer. bison	× Dom. cow	0	3	Rothwell (1924)
Amer. bison	× Dom. cow	2	3	Iwanow and Philiptschenko (1916)
Amer. bison	× Dom. cow	2	4	Deakin *et al.* (1935)
Dom. bull	× Bison cow	7	7	Deakin *et al.* (1935)
Europ. bison	× Dom. cow	1	3	Iwanow and Philiptschenko (1916)
		19	62	(23.5 ± 3.18)
Banting	× Zebu	1	0	Nathusius (1911b
Yak	× Sanga	1	0	Nathusius (1911b)
Gayal	× Sanga	0	1	Nathusius (1911b)
Zebu	× Yak	1	2	Zawadowsky (1931)
Yak	× Zebu	1	0	Mitchell (1849)
Yak	× Dom. cow	9	10	Kühn (Lotsy 1922)
Yak	× Dom. cow	1	5	Rothwell (1924)
Yak	× Dom. cow	3	8	Deakin *et al.* (1935)
Dom. bull	× Yak	1	0	Rothwell (1924)
Dom. bull	× Yak	1	1	Deakin *et al.* (1935)
Yak	× Bison	0	1	Rothwell (1924)
Yak	× Bison	0	1	Deakin *et al.* (1935)
Eland	× Afrikander	1	0	Warren (1932)
		20	29	(40.82 ± 4.73)
Zebu	× Gayal	0	3	Bartlett (1884)
Gayal	× Dom. cow	13	14	Nathusius (1911a)
Gayal	× Dom. cow	9	9	Kühn (1888)
		22	26	(45.8 ± 4.85 per cent males)
Zebu	× Dom. cow	12	9	Nathusius (1911b)
Dom. bull	× Zebu	4	2	Nathusius (1911b)
Zebu	× Dom. cow	4	2	Pucci (1914)
Bos taurus	× *B. indicus*	1244	1109	Col. Matson (*In litt.* 1931)
		1264	1122	(52.95 ± 0.69 per cent males)

Goodnight (1914) states, that he has never had any male hybrids born alive

from matings of the American bison bull with domestic cows. The cows carrying bull calves either aborted or the calf died at birth. He further points out that only a few females are obtained and when these were bred to bison bulls they produced fertile females and infertile males. But when mated to the domestic bull they gave fertile offspring of both sexes. Iwanow and Philiptschenko (1916) reported sterile males and fertile females, when hybrid females were mated to either bison or domestic bulls. These authors also report similar results when the European bison was mated to domestic cows.

The numbers in crosses involving the zebu (*Bos indicus*), yak, domestic cattle, also eland and Afrikander, are small, being only 20 males and 29 females. The sex ratio is 40.82 ± 4.73 per cent males, which is low but due to small numbers the difference may not be significant. Males from these crosses are reported as sterile, while the females appear to give reasonably high fertility. Zawadowsky (1931) reported successful matings for both of his female zebu-yak hybrids. Pucci (1914) mentioned a total of 113 matings (zebu and domestic cow) and it was pointed out that the matings gave 92 per cent fertility—but sexes were given for only 6 calves.

Hybrids reported from crosses of the gayal with zebu and domestic cattle total 22 males and 26 females. Thus the sex ratio is 45.8 ± 4.85 per cent males. Because of the small numbers on which the ratio is based the difference between this ratio and that normal for the genus is not considered significant. Again the males were usually found to be sterile. Nathusius (1911a), however, reported a gayal-domestic cow hybrid bull that produced 11 calves from 21 matings. The F_1 female hybrids recorded by Nathusius were continued in breeding until 140 calves were subsequently produced. The proportion of the sexes is essentially equal, there being 69 males and 71 females.

b. Sex ratio of *Bos taurus* × *B. indicus* hybrids

Through Colonel Matson unpublished records were obtained for the sexes of hybrids between *Bos taurus* males and *B. indicus* females. The parentage was known for 1244 males and 1109 females, which is 52.87 ± 0.69 per cent males. (Parentage was uncertain for an additional 165 males and 127 females.) Sexes previously recorded for this cross bring the totals to 1264 males and 1122 females. The sex ratio is, therefore, 52.95 ± 0.69 per cent males, which is slightly higher than the normal ratio reported for the parental species. Since the sex ratio for these hybrids is even higher than that for the parental species, the results are not in accord with the view that a deficiency of males is produced by hybridization or else this should not be considered a species cross. According to Morse (1910), Lydekker recognized this fact. He suggested that all of the forms without the hump should be designated *Bos taurus typicus* and all of those with the hump *Bos taurus indicus*. The crossing results certainly favor this interpretation of their relationship, and it is further supported by the fact that the progeny are apparently fully fertile with the parent species.

4. Sex ratio of miscellaneous hybrid mammals

A summary of the sexes reported for miscellaneous hybrid mammals is given in Table 4. Detlefsen (1914) obtained 14 males and 23 females in an F_1 from a wild Brazilian cavy male (*Cavia rufescens*) mated with domestic (*C. porcellus*) females. This is a ratio of 37.84 ± 5.38 per cent males. The normal sex ratio for the

TABLE 4

Sexes of miscellaneous hybrids

CROSS	M.	F.	SEX RATIO	AUTHORITY
Brazilian cavy × Dom. cavy	14	23	37.84 ± 5.38	Detlefsen (1914)
Mus bactrianus × *M. musculus*	104	101		Green (1930)
Mus musculus × *M. bactrianus*	29	31		Green (1930)
	133	132	50.19	
Goat × Ewe	0	5		Spillman (1913)
Goat × Ewe	13	4		Buffon (1791)
Goat × Ewe	1	4		Weir (1888)
Goat × Ewe	1	1		Kulaginn (1889)
	15	14	51.72	
Mouflon × Dom. Sheep	91	121	42.92 ± 2.29	Kühn (1888)
Kamerunschaf × Karakul	6	3		Heck, and Steinmetz (*In litt.* 1932)[1]
Karakul × Heidschnucke	0	1		Dr. Lutz Heck (*In litt.* 1932)[1]
	6	4		
Dromedar × Kamel	1	0		Dr. Lutz Heck (*In litt.* 1932)[1]
Wolf × Dog	4	4		Noack (1887)
Wolf × Dog	1	4		Anonymous (1883)
Wolf × Dog	1	1		Steinmetz (*In litt.* 1932)[1]
	6	9	40.0 ± 8.56	
European wolf × Dingo	0	6		Anonymous (1932)
Wild boar × Dom. sow	3	8		Spiller (1894)
Wild boar × Dom. sow	11	9		Culbertson (*In litt.* 1932)[2]
	14	17	45.16 ± 6.0	
Hare × Rabbit	0	1		Thursfield (1830)
Hare × Rabbit	0	1		Lönnberg (1905)
Hare × Rabbit	1	1		Kuiper (1925)
	1	3	25.0	
Mustela putorius × *Martes furo*	2	3		Pitt (1921)
Mustela erminea × *Mustela furo*	1	3		Cocks (1899)
Otaria pussila × *O. calif.*	1	0		Jennison (1914)
Ursus americanus × *U. arctos*	1	1		Bartlett (1860)

[1] Information furnished in letters from Dr. Lutz Heck and Dr. H. Steinmetz, Berlin Zoological Graden.

[2] Unpublished records Ia. Agr. Exp. Sta. through courtesy of Mr. C. C. Culbertson.

TABLE 4—*Concluded*

CROSS	M.	F.	SEX RATIO	AUTHORITY
Lemur macaco × *L. fulvus rufif.*	0	1		Pocock (1911)
Bubalus lunata × *B. caama*	1	0		Selous (1893)
Masai × Atbara-Pavian	2	0		Heck, and Steinmetz (*In litt.* 1932)[1]
Braungraue Meerkatze × Campel	1	0		Heck, and Steinmetz (*In litt.* 1932)[1]
Löwen × Tiger	1	0		Heck, (In litt. 1932)[1]
Barbary Ape × Mangaby Monkey	1	0[3]		
Mus rattus × *M. decumanus*	1	0		Van Kemper (1899)
Taurotragus oryx × *Strepsiceros capensis*	1	0		Boulineau (1933)
Taurotragus oryx × *Coudou*	0	1		Boulineau (1933)
Dog × Fox	1	.0		Priesner (1933)
Pariah-dog × Jackal	2	1		Hurst (1933)
	16	10		

[3] Information supplied by Mr. Fred Winkelmann, Supt. Vilas Park Zoo, Madison, Wisconsin.

guinea pig as reported by Ibsen (1923) is 51.67, Wright (1922) 50.4, Schott and Lambert (1930) 49.45 per cent males. Again the F_1 males were sterile and the females were fertile. Back-crosses were made by breeding the hybrid females to *porcellus* males, from which 31 males and 52 females were produced. These females mated to *porcellus* males gave 101 males to 116 females, and in the next cross 159 males to 153 females were produced. Detlefsen stated, that with each subsequent back-cross increasing signs of fertility of the males appeared.

Sexes for F_1 hybrids between two species of mice have been reported by Green (1930). The males total 133 and the females 132. Both sexes were fertile in back-crosses. Green's data show an increase in the proportion of males obtained from the back-crosses over that of the F_1, but the numbers are too small for significance.

Parkes (1924) reports a sex ratio of 118.0 (54.1 per cent) in the domestic mouse, but notes that Schultze (1903), with about the same numbers found approximate equality. Karol (1928), summarizing the results obtained by Sumner, on three subspecies of deer mice (*Peromyscus polionotus*), reports a sex ratio of 103.01 ± 1.64 (50.7 per cent) for the species as a whole. The ratio for subspecific hybrids in this species was 114.61 ± 5.79. Subspecific hybrids mainly of *P. maniculatus* gave a lower ratio. In general, however, it may be said that the differences in the ratios of the several species and of these subspecific hybrids are of doubtful significance. Apparently in these cases subspecific differences are not sufficient to affect seriously the sex ratio.

The goat and sheep crosses shown in Table 4 show a sex ratio as near equality as could be expected for only 29 individuals. Much doubt is entertained regarding this cross, although Buffon specifically states that he made the cross for those which he reported. The matings for the others are not vouched for by those who gave the reports. Kühn (1888) reported that in several hundred matings of goats with sheep no offspring was produced. Warwick, Berry and Horlacher (1934) reported 48 matings of rams with Angora female goats, in carefully planned and conducted experiments and although 22 conceptions occurred none of the embryos survived to the end of a normal gestation period.

Kühn (1888) observed 91 males and 121 females in an F_1 from crosses of mouflon and domestic sheep. By mating the hybrids he secured an F_2 with 43 males to 47 females, and an F_3 with 16 males and 11 females. The sex ratio for the F_1 is 42.92 ± 2.29 per cent. Although the numbers are small the sex ratio approaches equality in the subsequent generations. Numerous authors, notably Nichols (1924), White and Roberts (1927), Chapman and Lush (1932), and Johansson (1932b), have reported the sex ratio for domestic sheep to be slightly below equality (about 49 per cent males). Frolich (1936) reported observations on a flock of Karakul sheep which show a sex ratio of 52.3 ± 5.9 per cent. The numbers are small, however, being only 304.

The ratio for the F_1 hybrids in Kühn's experiments is about 6 per cent below that which appears to be the normal but the difference is not statistically significant. This, together with the fertility of the hybrids, rather clearly indicates that the low sex ratio found for this cross may be due to chance. If Kühn's conclusion that mouflon and domestic sheep belong to the same species is correct, a disturbance of the sex ratio would not be expected to result from crossing the two.

Reports of the sexes of the F_1 for crosses of the wolf and the dog show a total of 6 males and 9 females. While females are in excess the numbers are too small to be significant.

Two groups of hybrids between wild and domestic swine are reported. The hybrids were reported to be fertile and for the small numbers the sex ratio is probably not disturbed. Therefore, this is not likely a wide cross.

Discussions in the literature show controversy of long standing regarding the hare and rabbit cross. Nachtsheim (1935) has presented a comprehensive review of the conflicting views of this controversy. He points out that the few controlled experiments, including his own, have yielded negative results. Kuiper (1925) discussed Houwink's experiments and reported 13 F_1 hybrids for this cross. He also mentioned that 34 offspring were obtained from matings involving the F_1. Sexes were mentioned for only one litter, which contained one individual of each sex.

The numbers for the miscellaneous hybrids shown at the bottom of Table 4 are too small to indicate sex ratios. These are included with the hope that some readers may be interested in the particular matings involved.

DISCUSSION

While the primary sex ratio is not known for any mammal, such evidence as there is on foetal sex ratios shows a higher proportion of males in the earlier stages. This would indicate a still higher primary ratio, if the trend holds as the curve, plotting the sex ratio, is projected back to the time of conception, with many more males than females conceived. As already stated, the secondary ratio approaches equality and this could result only from a compensatory high death rate of males.

1. *The foetal sex ratio*

Records of foetal sex ratios are meager but for the most part those which have been reported show a significantly higher proportion of males than occurs at birth. Jewell (1921) observed the sexes for 1000 foetal calves and found a ratio of 55.2 per cent males. Crew (1925) and Parkes (1925) concluded from a study of the sexes of pig embryos that the foetal sex ratio for this species is approximately 60 per cent males. Ibsen (1928) observed the proportion of the sexes late in the

gestation period for the guinea pig and found 55.95 per cent males. Crew (1927) gave a comprehensive discussion of the sex ratio in man and pointed out that the ratio for abortions and still-births is distinctly higher than the secondary ratio. He concluded that the primary sex ratio for man must be at least 170 males to 100 females (about 63 per cent males). Furthermore, it is pointed out by Crew that the proportion of males decreases continuously during foetal life and until about the fifth year after birth. Parkes (1926) gave a summary of the sex ratios of aborted foetuses for several species and concluded that the sex ratios are appreciably higher than those for live-born young. He also observed that in several polytocous species the number of young born was less than the number of corpora lutea. Hammond (1914) found an appreciable difference in the number of corpora lutea and the number of foetuses present in both the sow and the rabbit. Corner (1923) observed that about 20 per cent of the ova in the sow were missing before the end of the fourth week of gestation, and a further loss occurred in some sows during the latter course of pregnancy so that at term about 25 to 30 per cent of the originally discharged ova were either totally missing, or the foetuses were degenerating or abnormal. MacDowell and Lord (1925, 1926) reported observations on counts of the number of corpora lutea in pregnant mice and young sexed immediately after birth and it was found that, in 106 litters where the number of corpora lutea exactly corresponded to the number of young born, the secondary sex ratio was equality. These authors maintain that the evidence for the conclusion that the male foetus is less viable than the female is incomplete, and they question the belief in a mammalian primary sex ratio being above equality.

Düsing (1887), in his studies of the sex ratio for the horse, found the number that died *in utero* or within one month after birth gave a sex ratio of 157 males to 100 females. This is a ratio of 61.09 per cent males. Data presented in the present study for still-born horse colts show a sex ratio of 77.5 ± 4.4 per cent males. But these data are not complete because there were 7 pairs of still-born twins for which sex was not recorded. Little is known regarding the completeness of Düsing's data. Furthermore, his ratio does not represent that of still-births alone since deaths for one month after birth are included. However, these data indicate that foetal mortality may fall heavily on males in this species.

Records presented for mule foals show a slightly higher percentage of still-births than those recorded for the horse. But the difference between the sexes of still-born mules is insignificant.

These facts are interpreted as indicating that males are conceived more often than females. Also that the equality of the secondary sex ratio is probably due to a higher mortality of male than female embryos. A foetal sex ratio above equality in mammals does not necessarily conflict with the modern chromosome theory of sex determination, since there are several possible ways to account for more male than female zygotes being conceived.

It has been frequently suggested that there may be a difference in the ability of the two types of spermatozoa to make successful contact with the ova, and that this might be accomplished by a difference in the rate of movement of the two types of spermatozoa toward the egg. If the Y-bearing spermatozoon is smaller and thereby lighter than the X-bearing it might be favored in the journey toward the egg. Some evidence in support of this suggestion has been reported.

Wodsedalek (1913, 1914, 1920) observed a size dimorphism in the spermatozoa of the pig, horse and bull. Parkes (1923) reported a size difference for the spermatozoa of man, the rat and the mouse. But Krallinger (1928), and Williams and Savage (1925) did not find this in their studies for the bull. Moench and Holt (1929) also failed to find a size difference in the spermatozoa of the pig and man. Zeleny and Faust (1915) observed a slight difference in the spermatozoa of the ram, bull and dog. Lush (1925) observed a slight size difference in the spermatozoa of the rabbit and the pig. He also attempted to separate the spermatozoa into two size groups for the rabbit by centrifuging the seminal fluid. Sex ratios obtained by artificial insemination with the centrifuged spermatozoa of the two size group samples were apparently unaffected.

Evidence obtained from double matings tends to show that there may be a differential "potency" of spermatozoa from different males. Cole and Davis (1914) demonstrated this with rabbits by simultaneous matings of a pigmented and an albino male to albino females. In 23 matings 190 young were born of which 166 were by the pigmented male. For 15 of the 23 litters the spermatozoa from the pigmented male apparently succeeded in fertilizing all of the eggs. The difference was probably not due to actual low fertility of the albino male, for when used alone he gave good fertility. As pointed out by the authors this might be accounted for on the basis of a difference in the vitality of the spermatozoa, expressed in a greater or less motility or a difference in the penetrating power of the gametes of the two males. Similarly, such a difference might exist between the two different types of spermatozoa produced by a single male. Thus an excess of one sex might be expected.

King (1918) in discussing the difference between the sex ratios of two strains of rats produced by inbreeding and selection states that the results seem to show that in the rat the female has a greater influence than the male in determining the sex ratio. Furthermore, King suggested that the greater the difference of blood relationship between individuals the stronger is the attraction between the ova and the male-producing spermatozoa. Obviously a low sex ratio among interspecific hybrids does not harmonize with this suggestion.

Since the available evidence indicates a larger proportion of males than females among foetuses and the ratio at birth approaches equality, there must be a disturbance of some sort that is responsible for a higher death toll among male embryos.

A few hypotheses have been suggested. It has been mentioned frequently that sex-linked lethal genes may be operative in placing a greater mortality on the male sex, because of its possession of only one X-chromosome. MacArthur and Baillie (1932) object to this on the ground that the sex mortality relations expected in avian and lepidopterous forms on this theory is not found. Furthermore, they maintain that it is not clear that the theory even explains the mortality relations in *Drosophila* type forms. They state that, considering the relative number and size of the sex chromosomes, fewer sex-linked genes, and less sex-differential mortality, would be anticipated in man than in *Drosophila*, and in both less than in *Hymenoptera*, where haploid males may receive unfavorable recessive factors in each chromosome. But the possibility remains that sex-linked lethal genes may be a factor affecting foetal mortality of males in mammals.

Riddle (1927) points out that the serological studies at the Frauenklinik at

Halle indicate that the blood of the mother reacts to a male foetus as to a foreign body, and that an "antitestis" substance may enter the foetal blood and induce a reaction in the male foetus. He also states that the mother's blood does not appear to react similarly against a female foetus, and that the intrauterine environment is not equivalent for the male and the female embryo. Lillie (1917) offered the suggestion that a disturbance of the equilibrium that protects the male from the sex hormones of the mother during pregnancy may result in malformations of the male sex characters and that this might cause a greater mortality among male foetuses. A somewhat similar hypothesis was advanced by Kostoff (1931) to account for unsuccessful interspecific matings. It was suggested that antibodies are produced by the maternal organism, against hybrid embryos, which may result in sterile matings. The hypothesis was confirmed by immunological experiments with plants but the results were less positive in tests with animals.

MacArthur and Baillie (1932) gave a comprehensive discussion of the differences in the death rates of the sexes for numerous species, and presented evidence which indicates that as a rule the death toll is greater on the male than on the female sex in all groups of animals, irrespective of their homogamety or heterogamety. Numerous researches have shown that for several different species the male has a higher metabolic level than the female. Riddle pointed out that this would involve increased nutritional and oxygen demands and might therefore result in a higher prenatal death-rate on the male side.

Gowen (1931a, 1931b), working with *Drosophila*, observed that individuals possessing unbalanced groups of chromosomes had a shorter life span than those that had balanced groups. Furthermore, he reported that qualitatively the larger the production of CO_2 per day the shorter the time the flies were capable of living. Flies with balanced chromosome groups produced less CO_2 than those with unbalanced groups.

Should maleness itself impose a higher metabolic level than femaleness and chromosome unbalance tend to accentuate the rate of metabolism, it appears possible that the rate of metabolism might become a more deleterious force among males in mammals, since that sex may have a less favorable chromosome balance due to its having only one X-chromosome. If antibodies or hormones produced by the mother were the only factors affecting foetal mortality, a deficiency of males might be expected among birds, since eggs are known to contain maternal hormones. Although the sex ratio among birds has been reported slightly below equality some reports show an excess of males, and among hybrid birds it is the female sex that shows a deficiency in numbers. Therefore, the view that female hormones or antibodies are responsible for a greater embryonic death toll of males than females does not harmonize with the facts for both mammals and birds.

In view of the evidence which various researches have yielded on the question of prenatal mortality and sex ratios it appears improbable that one factor alone can be relied on to account for a higher death toll among male than female embryos in all cases. Apparently, at least three factors may be involved. First, sex-linked lethal genes may disturb the normal proportion of the sexes in some instances; second, since the Y-chromosome apparently does not as a rule carry genes the male (or heterogametic) sex may receive fewer dominant genes than the female (or homogametic sex) in some cases,

and in such cases the male may actually lack the vigor possessed by the female; third, evidently maleness imposes a higher rate of metabolism than femaleness, and in heterogametic males this may become a deleterious force, since the possession of only one X-chromosome might result in a less favorable gene balance which may accentuate the metabolic rate.

All of these factors may be operative to a varying degree in interspecific and intergeneric crosses. In certain crosses there may be a pronounced degree of unfavorable action due to these forces and it is possible that a markedly disturbed sex ratio would result in such instances. In other crosses only one or two of these factors may be involved, to a lesser extent, and in such cases the disturbance may be imperceptible or slight. Since in general in crosses of widely separated species there is a greater disparity between the numbers of the sexes than for more closely related species it appears that chromosome unbalance may be the disturbing factor of most importance.

Furthermore, chromosome unbalance may be so great in some crosses that even the homogametic sex can not survive. In such cases the matings would be completely sterile.

2. *Sterility of hybrids*

Sterility among hybrid mammals appears to be much more frequent in males than females. It has been pointed out by numerous observers that sterility results from chromosome incompatibility in hybrids in the reduction phases of gametogenesis. The degree of disturbance may be less in the female hybrid because all members of the pairs of chromosomes have similar mates in this sex. Furthermore, irregular chromosome behavior in meiosis may occur more often in the heterogametic sex because of a greater dissimilarity of the X- and Y-chromosomes; but still the exact cause is not clear. As pointed out by Huskins (1929) and observed by Clausen (1930) abnormal chromosome behavior may occur within a pure species where the members of each pair, as far as is known, are homologous. But the possibility of chromosome or gene balance as a factor effecting fertility in hybrids appears to remain.

3. *Criteria of hybridity*

Data secured in this study emphasize the need for a more reliable criterion of hybridity than we now have. Sterility of the hybrids was once thought to be a very definite rule, but this does not fully suffice since sterility may occur in only one sex in some cases and in both in others. Furthermore, sterility may occur within a breed, or within a strain of a breed. However, in general sterility may indicate hybrid relations and it is therefore still considered one of the indices of hybridity. Irregularities in meiosis may be characteristic of hybridity as has been pointed out particularly by Jeffrey (1928), Wodsedalek (1916), Zuitin (1930), Renner (1924) and Federley (1923). However, Huskins (1929), objects to irregularities in meiosis as a criterion because some interspecific hybrids may show regular maturation divisions and still be sterile; and because irregularity of meiosis can be induced by agencies other than hybridization. Serologic tests were invoked by Landsteiner and van der Scheer (1924), and Walsh (1924). The more recent work of Irwin, Cole and Gordon (1936), Irwin and Cole (1936a) and (1936b) on immunogenetic relations in birds demonstrates further the applicability of this test as a criterion of hybridity.

4. *Fertility of the mule*

Cases of fertile female mules have been recorded by numerous observers notably Hensler (1925), Jones (1916), (Jones also

reported a fertile female hinny), Braas-huus-Jessen (1930), Haworth (1928), Goldsmith (1917), Ewart (1910b), Eichorn (1929), Warren (1926) and Groth (1928), Warren (1933), and Gramlich (in *litt.* to author, 1936). In some of these cases the observers expressed doubt as to the mother truly being a mule, or as to the young actually being from the animal claimed to be the mother. But the case reported by Groth was subjected to the serologic test, and the blood of the mother gave the reaction characteristic of mules. Furthermore, this mule produced young while in the possession of the Texas A. & M. College, where critical attention was given to mating and foaling. It appears that this case should establish the fact that female mules may in some cases be fertile.

So far as known to the writer no case of fertility of the male mule has been recorded. Histological studies of the testis of the male mule have been reported by Stephan (1902), Whitehead (1908), Wodsedalek (1916), and Goldsmith (1917). Various disturbances which indicated complete sterility were found. Ewart (1910b) observed the seminal fluid from one of his zebra-horse hybrids and found tailless or almost tailless spermatozoa which were immobile.

5. *General remarks*

Crosses involving bison and domestic cattle result in a markedly disturbed sex ratio, and fertility of the female hybrids is low, while the males are completely sterile. Therefore, the facts appear to favor the classification of these two forms in a separate genus as is now the case. Although the yak is placed by some zoologists in the genus *Poephagus*, the fact that the sex ratio of hybrids from crosses of the yak with species of the genus *Bos* is not seriously disturbed and female hybrids appear to give good fertility, and that males are occasionally fertile, tends to favor the classification of this form in the genus *Bos*. Apparently American zoologists generally agree that it belongs in this genus.

The sex ratio and the fertility of the hybrids from crosses of Zebu and domestic cattle is in perfect harmony with the modern view that these animals belong to the same species.

The two species of mice (*Mus bactrianus* and *M. musculus*) crossed by Green (1930) gave a sex ratio essentially equal to that normal for the parent species. The hybrids were also fertile. Therefore, the question of a true specific difference between these two forms is raised.

When all the known criteria of hybridity are applied to the mule, evidence that the horse and the ass belong to different species is convincing. However, the two species are known to cross with little difficulty and the issue of the cross (the mule) has proved to be of unusual value. As a result of the crossability of the two species and the utility of the hybrid, mules have been produced since early times. The cross is commonly made only one way in the United States; namely, by mating jacks to mares. It is rather generally claimed that the issue of the reciprocal cross (the hinny) is more horse-like than the mule, and lacks the hardiness characteristic of the mule. But this is doubted by some observers. Definite proof for either view seems to be lacking.

SUMMARY

1. Individual hybrids between different species and genera of mammals, for which sex is recorded by various observers, are summarized. On the whole the sex ratio is appreciably below equality, i.e. the females are in excess.

2. Original records on the sexes of 1416 mule foals sired by 98 different jacks show a sex ratio of 44.28 ± 0.89 per cent males.

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THE METABOLISM OF PATHOGENIC TRYPANOSOMES AND THE CARBOHYDRATE METABOLISM OF THEIR HOSTS

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INTRODUCTION

TRYPANOSOMES are parasitic protozoa that live in the blood of a great variety of species in all classes of vertebrates and that are generally transmitted by an invertebrate intermediary host. A characteristic morphological feature of the forms that live in the blood is an undulating membrane along which runs a flagellum. This terminates at the posterior end in the blepharoplast, the kinetic center of the cell, and at the other end in a free flagellum. In many cases, different species are so much alike that they cannot be distinguished morphologically, but only by means of biological experiments. In other cases, they can readily be recognized by morphological peculiarities. A few types, which may illustrate this, are shown in Fig. 1. Most of the species, for example *Trypanosoma rotatorium*, the first described trypanosome (Gluge, 1842), and all the others occurring in fishes, amphibians, reptilians and birds are without injurious effects on their hosts. The same is true for most trypanosomes which live in mammals. But the group is nevertheless of great interest both from a public health and an economic standpoint, since some very pathogenic trypanosomes are found in man and domesticated animals. Human sleeping sickness in Africa is caused by *Trypanosoma gambiense* and *T. rhodesiense*, the former being the first trypanosome found in man, and first described by Dutton. In South America *Trypanosoma cruzi* is, according to Chagas, responsible for a human disease. *Trypanosoma brucei*, *T. evansi*, *T. equiperdum*, *T. equinum* and some others cause severe diseases of domesticated animals in various parts of the world. It is therefore not astonishing that many investigations have been carried out on the various aspects of the biology of these pathogenic forms. In the present paper the metabolism of the pathogenic trypanosomes and the problem of the relationships of this metabolism to the injuries suffered by the host during the infection are reviewed.

SCHERN'S "REVIVING-PHENOMENON"

Schern (1912) showed that it was possible to revive trypanosomes *in vitro*, which had lost their motility, through the addition to the medium of serum or liver-extract of normal animals, but not of animals suffering from heavy trypanosome infections. Because this observation suggested possible implications concerning the mechanism of pathogenicity of the trypanosomes it gave during the last decade the impulse for many investigations concerning both the metabolism of the host and of the parasites.

Schern's reviving-phenomenon was apparently due mainly to the presence of glucose in the serum and liver-extract. It was possible to get a similar effect with glucose, levulose, mannose or glycerol, whereas galactose and maltose were less favorable and many other substances had no influence on the viability of the parasites (Schern, 1912, 1925, 1928; Kudicke and Evers, 1924; Dubois, 1926, 1930; von Jancsó and von Jancsó, 1935). These findings, combined with the demonstration that both *Trypanosoma lewisi* and pathogenic trypanosomes survived longer *in vitro* in the presence of carbohydrates (Biot, Biot and Richard, 1911; Fleig, 1911; Poindexter, 1935) and that glucose is essential in culture media used in the maintenance of many trypanosomes (first recognized by Hagemeister, 1914), suggested that the sugar is used by the parasites. The experiments mentioned, however, were not definitive proof that this assumption is justified, since von Fenyvessy (1926) could obtain Schern's reviving-phenomenon with sugar-free solutions,—for example, sugar-free broth—and since Dubois and Bouckaert (1927) and Bruynoghe, Dubois and Bouckaert (1927) tried without success to demonstrate the actual sugar consumption *in vitro*.

METABOLISM OF TRYPANOSOMES

Sugar metabolism of the trypanosomes. Using reliable quantitative methods, however, Yorke, Adams and Murgatroyd (1929) showed for the first time that the trypanosomes use really large amounts of sugar *in vitro*. These findings have been confirmed subsequently by different authors (Regendanz, 1930; Geiger, Kligler and Comaroff, 1930; von Fenyvessy and Scheff, 1930; von Issekutz, 1933; von Brand, 1933; Reiner and Smythe, 1934; von Jancsó and von Jancsó, 1935; Reiner, Smythe and Pedlow, 1936), It has been shown that the pathogenic trypanosomes, with the exception of *Trypanosoma cruzi*, consumed quantities of sugar amounting to about 7–8 mg. per 1000 millions animals per hour at 37°C., whereas the non-pathogenic *Trypanosoma lewisi* consumed under similar conditions only about 1.5 mg. of sugar. The intensity of carbohydrate metabolism is therefore extremely

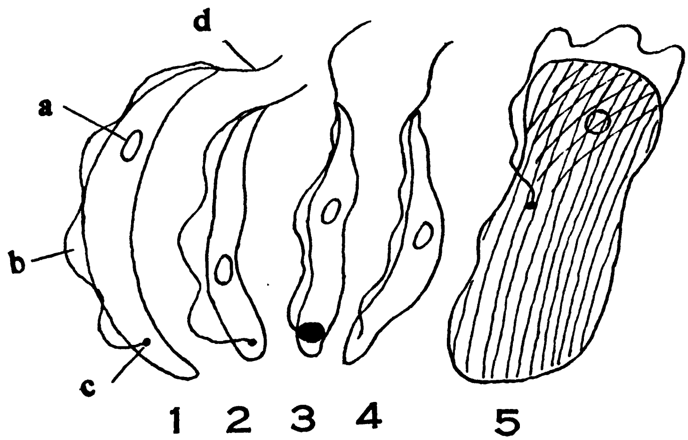

FIG. 1. VARIOUS TYPES OF TRYPANOSOMES

(1) *Trypanosoma lewisi*, nucleus in the anterior part of the body. (2) *T. gambiense*, nucleus often in the posterior part of the body. (3) *T. cruzi*, characterized by the big blepharoplast. (4) *T. equinum*, characterized by having no blepharoplast. (5) *T. rotatorium*, broad form with striped pellicula. (*a*, nucleus, *b*, undulating membrane, *c*, free flagellum, *d*, blepharoplast.)

high. This is strikingly evident if we consider the fact that as 1000 millions of trypanosomes correspond to approximately 60 mg. of fresh substance only, they consume during 24 hours about twice their own weight of sugar, whereas the energy requirement of a 70 kg. man for the same period is contained in about 500 gm. of sugar. This difference is probably due to three facts: (1) The trypanosomes are extremely active organisms. (2) They are very small and their relative surface is much greater than that of man, and (3) they utilize only a fraction of the energy contained in the carbohydrate, since they do not totally oxidize the sugar to carbon dioxide and water.

The rate of sugar consumption is, as in other animals, in a high degree dependent upon the temperature (Regendanz, 1930), the temperature curve resembling Krogh's normal curve more than a van't Hoff curve (von Brand, 1933). Opinions are divided as to whether the trypanosomes are able to maintain the same level of carbohydrate consumption in media of different sugar concentration. Regendanz (1930) and von Jancsó and von Jancsó (1935) found the carbohydrate metabolism less pronounced in lower sugar concentrations than in higher ones and the same was reported for very low concentrations by von Fenyvessy and Scheff (1930); von Brand (1933), on the other hand, observed a uniform rate of sugar consumption at least in a range of concentrations which may occur in the blood of an infected animal. The same difference of opinions is based upon experiments *in vivo*. Krijgsman (1933) could obtain no changes in the course of the infections by injecting his experimental animals with insulin, indicating that enough sugar was always present in the blood for the parasites. Poindexter (1935), on the other hand, states that in animals subjected to insulin treatment the infection lasted longer than in the controls and fewer parasites were present. He assumes that this was due to the lowering of the blood-sugar level, which would make less favorable conditions for the development of the trypanosomes.

The quantitative experiments mentioned above were carried out with glucose, but this is not the only sugar which can be used. Of the pathogenic trypanosomes only *Trypanosoma brucei* has been examined quantitatively in relation to its consumption of other carbohydrates. It was found (von Brand, 1933) that this organism could satisfy its sugar requirements only with glucose, mannose, maltose, levulose and galactose. The corresponding relative consumption rates were 100:86:50:21:9.

Apparently trypanosomes in general do not accumulate carbohydrate reserves in their body in the form of glycogen (Krijgsman, 1936), though Schern and Bozzolo (1930) seem to have found a small amount of glycogen. This question is not of great importance, since even relatively large reserves would last only a very short period because of the enormous intensity of the carbohydrate metabolism of the parasites. It seems that the blood-inhabiting forms of the pathogenic trypanosomes are absolutely dependent upon the presence of an available sugar in their surroundings. It is probable that sugar is their only source of energy (v. Brand, 1933, Krijgsman, 1936, von Jancsó and von Jancsó, 1935). Whether this is true also for their developmental stages is not yet sufficiently investigated (von Brand, 1935).

Oxygen consumption and end-products of sugar metabolism. As might be expected from blood-inhabiting organisms, the trypanosomes use oxygen as first shown by Nauss and Yorke (1911) and confirmed by several authors (von Fenyvessy and Reiner,

1924; von Fenyvessy and Scheff, 1930; von Issekutz, 1933; Reiner and Smythe, 1934; Reiner, Smythe and Pedlow, 1936). It was remarkable, however, that the amount of oxygen consumed was too small to account for the total oxidation of the sugar. The necessary consequence is that the end-products resulting from the carbohydrate metabolism, are only partially oxidized substances. It was first observed that during experiments *in vitro* carbon dioxide was liberated from bicarbonate and that the pH of the medium was lowered (von Fenyvessy, 1926; von Fenyvessy and Reiner, 1928; Kligler and Geiger, 1928; Kligler, Geiger and Comaroff, 1929; Geiger, Kligler and Comaroff, 1930) thus indicating the production of a relatively strong acid. Since the lactic acid content of the blood of animals infected with trypanosomes was found to be higher than normal and since the chief end-product of anaerobic carbohydrate metabolism in many animals is lactic acid, it was assumed that the same acid was also produced in this case. Subsequent investigations, however, (von Brand, Regendanz and Weise, 1932; Reiner and Smythe, 1934; Reiner, Smythe and Pedlow, 1936) have shown, that the parasites in question give off no lactic acid, or at least only very small amounts. It was apparent that one or more other acids were formed, indicating that the type of carbohydrate metabolism is rather related to that of parasitic worms than to vertebrates (von Brand 1934, 1935). Reiner and Smythe (1934) and Reiner, Smythe and Pedlow (1936) could follow the whole course of glucose breakdown in the case of *Trypanosoma equiperdum*. The first step under aerobic conditions and the only one realized under anaerobic conditions was the transformation of one molecule of glucose to one molecule of glycerol and one molecule of pyruvic acid. The next step was the oxidation of glycerol to pyruvic acid and water. The whole process is represented by the following formulae:

$$(1)\ C_6H_{12}O_6 \rightarrow CH_3\ CO\ COOH + C_3H_8O_3$$

$$(2)\ C_3H_8O_3 + O_2 \rightarrow CH_3CO\ COOH + 2\ H_2O$$

It follows that under aerobic conditions one molecule of glucose was decomposed to two molecules of pyruvic acid. It is obvious that this scheme leaves no place for carbon dioxide and lactic acid. As a matter of fact traces of both substances were found, but they may have originated from the metabolic activities of leucocytes, the presence of which is difficult to avoid completely in experiments of this type. It is of great interest that according to Reiner, Smythe and Pedlow (1936) the end-products of sugar metabolism are very different in the case of the non-pathogenic *Trypanosoma lewisi*. They identified as end-products under aerobic conditions succinic, acetic and formic acid, ethyl alcohol and carbon dioxide.

Fat- and protein-metabolism of the trypanosomes. There is no evidence that the pathogenic trypanosomes can use fats, since no fat-splitting ferments could be found (Califano and Gritti, 1930; Krijgsman 1936). Protein metabolism also seems to be relatively unimportant (von Brand, 1933; Krijgsman, 1936) and is probably, at least for the greatest part, confined only to the transformation of the protein substances taken from the blood of the host into the parasite protoplasm during the growth period. It is of interest that of ferments acting on proteins only cathepsine, carboxylpeptidase, amino polypeptidase and dipeptidase could be found, but no pepsine or trypsine, which are so widely distributed in other animals (Krijgsman, 1936). This is connected with the fact that the trypanosomes live in an environment in which enough pro-

tein degradation products are present to fulfill their nitrogen requirements.

CHANGES IN SUGAR METABOLISM OF HOST

Behavior of blood sugar during infection. As we shall see later on, it has been attempted several times to correlate the sugar metabolism of the trypanosomes with changes of the carbohydrate metabolism of the host and to explain in this way the lethal effect of the pathogenic trypanosomes on their hosts. In order to establish or test these theories many investigations have been carried out on the carbohydrate metabolism of parasitized vertebrates. Before we go into the details of these theories, we shall consider first some actual results of these experiments.

The first thoroughly investigated item was the behavior of the blood-sugar throughout the infection. Different animals were used as hosts: rat, guinea-pig, rabbit, dog, cat, sheep, horse and monkey. From man, also, a few data are known. The results of these observations are not completely uniform. In a few cases (Cordier, 1927; Savino, 1927; Tubangui and Yutuc, 1931; Walravens, 1931; Wormall, 1932) no noticeable changes were found, in others (Schern, 1928; Schern and Rossi-Lema, 1928; Andrews and Sanders, 1928; Angolotti and Carda, 1929; Scheff, 1932) a hyperglycemia was described for the whole course or at least for a part of the infection. In most cases, however, it was noticed that the blood-sugar level was lower than normal. This hypoglycemia occurred generally only in the last days or hours of a lethal infection (Schern, 1925, 1928; Regendanz and Tropp, 1927; Cordier, 1927; Dubois and Bouckaert, 1927; Bruynoghe, Dubois and Bouckaert, 1927; Knowles and Das Gupta, 1927/28; Zotta and Radacovici, 1929; Scheff, 1928, 1932; Linton, 1930; Locatelli, 1930; von Brand and Regendanz, 1931; Tubangui and Yutuc, 1931; von Brand, Regendanz and Weise, 1932; Krijgsman, 1933; von Jancsó and von Jancsó, 1935), but in a few cases the lowering of the blood-sugar was observed during longer periods or even throughout the greater part of the infection (von Fenyvessy, 1926; Dubois and Bouckaert, 1927; Cordier, 1927; Scheff, 1932; Poindexter, 1935). The totality of these experiments certainly indicates a disequilibrium in the regulation of the blood-sugar of an infected animal, most pronounced in the later stages of the infection and primarily characterized by the premortal hypoglycemia. The disturbance in the blood-sugar regulation is more significant in those cases, in which distinctly lowered values are observed at least some days before the death, whereas a hypoglycemia occurring only a few hours before the animal dies, seems to be of little importance only. Concerning this latter point it must be borne in mind that similar observations have been made in bacterial infections (Zotta and Radacovici, 1929) and in infections with *Bartonella muris* (Linton, 1929; Regendanz, 1929; Hoffenreich, 1932; von Brand, Regendanz and Weise, 1932) thus indicating that it is not specific for trypanosome infections.

Polysaccharide reserves of the host. This was soon recognized and attempts were made to go further into the carbohydrate metabolism of the host by studying the behavior of the polysaccharide reserves. Schern and Verokay (1925) and Schern and Bozzolo (1930), using morphological methods, recognized that the glycogen content was greatly reduced, or that the reserve polysaccharide was even absent in various organs of animals which had died of trypanosomiasis. Other authors tested the glycogen content of the host with quantitative methods and came essentially to the same conclusion. At least in the

late stages of the infection the glycogen content of the liver was almost invariably much lower and that of the muscles in most cases lower than normal. In a few cases it was even absent (Regendanz and Tropp, 1927; Bruynoghe, Dubois and Bouckaert, 1927; Linton, 1930; von Brand and Regendanz, 1931; Scheff, 1932; Krijgsman, 1933). Only in one dying monkey the liver glycogen was still high (Regendanz, 1929) and according to Bruynoghe, Dubois and Bouckaert (1927) even more glycogen was found in the muscles of infected than in uninfected mice and rabbits. These observations suggest again that the carbohydrate metabolism of the host is not normal. We shall find further proofs in considering the theories concerning the genesis of this disturbance to which we shall now proceed.

THEORIES CONCERNING THE LETHAL MECHANISM OF TRYPANOSOMES

Sugar consumption theory. Schern (1925, 1927) was the first who tried to explain the pathogenic action of the trypanosomes chiefly by their carbohydrate metabolism. The sugar consumption of the parasites is in his opinion so high that they rapidly use up the sugar present in the blood of the host. This process is so intensive that it is difficult for the liver to mobilize enough sugar from its glycogen deposits to maintain the blood-sugar level and therewith the carbohydrate metabolism of the body normal. The consequence of this hypernormal function is a great strain on the liver which finally can fulfill its other duties (detoxicating action, etc.) no longer. Eventually the liver-function breaks down and causes a glycopryvic intoxication of the body, from which death ultimately results. According to this theory (see also Schern and Artagaveytia-Allende, 1936) the sugar consumption of the parasites would be directly responsible for the death of the host. The fact that death occurs sometimes when there are still noticeable amounts of glycogen present in the liver is explained by Schern (1930) by assuming that in these cases glycogen can be mobilized no more (see also Andrews, Johnson and Dormal, 1930).

This last point has, however, never been proved experimentally, but there are, on the other hand, clear indications that the liver-function is disturbed. Schern and Citron (1913) found that infected rats excreted levulose in the urine after consuming such small amounts of this sugar only that in comparably treated normal animals no glycosuria occurred. Scheff (1932) found that under normal conditions the feeding of sorbite caused no changes of the blood-sugar level, whereas in infected animals a typical blood-sugar curve was found. He states further that the blood-sugar curve resulting from oral administration of sugar had a different form in infected than in normal animals. In view of the great variabilities of the blood-sugar curves in normal animals von Brand and Regendanz (1931) think that further evidence is necessary before this point can be settled definitively. These authors showed that the ability of the liver to synthesize glycogen from sugar was greatly diminished in infected animals. In some cases no glycogen formation at all could be found, even after great sugar doses. These deficiencies in the liver-function have a morphological basis in pathological changes of the liver tissue, which could be observed in several cases (Battaglia, 1926; Andrews, Johnson and Dormal, 1930; Hoeppli and Regendanz, 1930; Scheff, 1928). These points can doubtless be used in favor of Schern's opinions and his theory has been accepted by some authors (von Fenyvessy, 1926; Scheff, 1928, 1932; Knowles and Das Gupta, 1927/28), but the majority of the investigators doubt

that the sugar consumption of the parasites is directly responsible for the pathogenicity of the trypanosomes.

Asphyxiation theory. Andrews and Johnson (1929/30) and Andrews, Johnson and Dormal (1930) concede that the carbohydrate metabolism of the parasites might be responsible for the lowering of the blood-sugar level, but they believe that the liver lesions and the death of the host are rather due to asphyxiation. This is caused by pulmonary edema brought about by partial obstruction of the circulation by the agglutination of parasites in the blood vessels of heart and lungs.

Lactic acid theory. The idea that asphyxiation might be the ultimate cause of death had already been expressed by Kligler, Geiger and Comaroff (1929), but they assumed a very different mechanism. In their opinion great amounts of lactic acid are formed in the blood by the parasites during their sugar metabolism. This acid would effectively interfere, owing to a specific influence on the haemoglobin, with the oxydative processes of the body and the result would eventually be asphyxiation. There is no doubt that at least in the last stages of a trypanosome infection the lactic acid content of the blood is generally markedly increased. This has been shown repeatedly both with direct and indirect methods (lactic acid determinations, lowering of the pH or alkaline reserve of the blood) by Kligler and Geiger, 1928; Scheff, 1928; Kligler, Geiger and Comaroff, 1929; Dominici, 1930; Linton, 1930; Andrews, Johnson and Dormal, 1930; von Brand, Regendanz and Weise, 1932; Krijgsman, 1933. As stated above, however, the trypanosomes produce no lactic acid. Its accumulation in the blood is therefore obviously due to the metabolic disequilibrium of the host and is by no means linked directly with the sugar metabolism of the parasites. Krijgsman (1936) points out that the concentration of the lactic acid is too small as to injure the host.

Toxin theory. The last group of investigators (Regendanz and Tropp, 1927; Zotta and Radacovici, 1929; Locatelli, 1930; von Brand and Regendanz, 1931; Krijgsman, 1933, 1936) denies the possibility that the changes in the metabolism of the host and its death can be explained by the sugar consumption of the parasites or by the action of the hitherto defined end-products of their carbohydrate metabolism. These authors believe that it is necessary to assume the production of other as yet uncharacterized end-products of metabolism, which may be called toxins. They use the following arguments. The observed hypoglycemia cannot be explained by the sugar consumption of the parasites, since it occured also in animals; for example, rabbits which generally harbor so few parasites that their sugar consumption cannot play any great role. It was impossible to prevent the hypoglycemia of the host even by administration of large amounts of sugar. It has been shown, furthermore, that an infected animal had in most cases enough carbohydrate in its body to raise the blood-sugar level under experimental conditions. With the exception of Scheff (1932) all investigators (Regendanz and Tropp, 1927; Regendanz, 1929; Krijgsman, 1933) could observe an elevation of the blood-sugar level in hypoglycemic animals after injection of adrenalin. This view seems to be substantiated by the experiments of Scheff (1932) and Bruynoghe, Dubois and Bouckaert (1927), who observed a rapid rise of the quantity of blood-sugar to the norm or even above after killing of the trypanosomes by means of medicaments, even in starving animals (Scheff 1932). Infected animals also die, when they are fed with sugar (Cordier, 1927; Bruynoghe,

Dubois and Bouckaert, 1927; Andrews, Johnson and Dormal, 1930). Sometimes they lived a little longer (Dubois, 1928; Kligler, Geiger and Comaroff, 1930; Angolotti and Carda, 1929), in other cases the parasites seemed to have been stimulated by the sugar and the death of the host occured even earlier than in controls (Poindexter, 1933). Finally von Brand (1933) pointed out that the intensity of the carbohydrate metabolism of the parasites, though surprisingly high, is not high enough to allow the assumption of an effective interference with the metabolism of the host. He calculated that in a human being suffering from sleeping sickness the parasites consume at the most 2–3 per cent of the calories which are required for a resting man. In a rat the figure is, for the last days of an infection about 30 per cent, but a rat can doubtless starve several days without dying. It must further be borne in mind that experimental animals infected with *Trypanosoma cruzi* die, though according to von Brand (1933) this species consumed probably even less sugar than the non-pathogenic *Trypanosoma lewisi*.

The above discussion shows that the arguments which have been brought forward in favor of the toxin theory are based so far on negative indications. It must be admitted that the various attempts to actually demonstrate the existence of toxins in trypanosomes have failed. But it must be pointed out that these experiments have been designed to show the presence of poisonous substances in the body of the parasites (endotoxins). Whereas it is possible that the active substance is excreted by the parasites (Krijgsman, 1933). In his last papers Krijgsman (1936) states that the toxin might possibly belong to the amines which are known to be very poisonous and the formation of which he has shown to be likely by finding corresponding ferments. If this assumption could be proved, it would follow that the disturbance of the carbohydrate metabolism of the host has no connection with the sugar metabolism of the parasites, but with their protein metabolism.

SUMMARY

As a summary of the main results of the experiments reviewed above, the following points may be emphasized:

1. The carbohydrate metabolism of the pathogenic trypanosomes is characterized by its great intensity and also by the fact that the sugar is only partially oxidized in the presence of oxygen.

2. The carbohydrate metabolism of a mammal infected with pathogenic trypanosomes is disturbed, as shown mainly by the disequilibrium of the blood-sugar, lowering of the glycogen reserves and reduced ability to build glycogen from sugar.

3. Four theories have been presented to explain the disturbance of the carbohydrate metabolism of the host and its eventual death, three of which involve metabolic interrelationships of parasites and host. According to one the loss of carbohydrate suffered by the body from the sugar consumption of the parasites is responsible for the injuries of the host, another claims that a supposed end-product of the sugar metabolism of the trypanosomes (lactic acid) is the causative factor. The third theory assumes that toxins, perhaps originating from the protein metabolism of the parasites, play the decisive role in the pathogenicity of these organisms.

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THE GENETICS OF CANCER IN MICE

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IT HAS been known for many years that an important aspect in the susceptibility to different forms of cancer is that which deals with hereditary factors. The nature of this inheritance is still unsettled in many respects.

The purpose of this article is to tabulate the results of the various investigations and to place before the reader the different theories on the inheritance of cancer.

Since most of the results have been explained in terms of genetics, the principles of that science should be applied to any experiment, not only in regard to the material used but also to the interpretation given. Perhaps the most important of these principles to be considered in cancer experimentation work is that of the homogeneity of the stocks. In working with animals it is usually considered that a stock has attained a high degree of homozygosity after fourteen generations of brother-to-sister matings. This method of mating is employed by most geneticists.

The difficulty of obtaining homogeneity is emphasized in the following quotation taken from East and Jones (1919):

> Although nearly complete homozygosity is theoretically brought about by seven generations of self-fertilization, the attainment of absolute homozygosity is a difficult matter and in practice it may never be reached. . . . Continued selective mating is necessary to bring about homozygosity. Intermittent inbreeding alternating with periods of outcrossing, which is the prevailing state of affairs with many organisms, cannot maintain any high degree of homozygosity.

Several observations may be cited to point out the consistency of the cancer incidence obtained in several inbred strains of mice. Strong (1934) found that selection within the Strong "A" strain had no influence on the age incidence of breast cancer. Selection was started after the 28th generation of inbreeding. In regard to another stock known as the C_3H race Strong (1935b) also stated that, provided the individuals belonged to the same inbred strain, the difference between an animal which developed cancer and a non-cancerous individual was probably due to chance alone or to some intercurrent disease.

Breeding tests made by MacDowell and Richter (1935) showed that animals of the strain C58 with doubtful diagnoses of leukemia had the same genetic constitution as animals with positive diagnoses. Their results are given in the following table.

MacDowell and Richter found that the proportion of positive diagnoses among the offspring of leukemic parents was 88.8 per cent. In two other classes, + × ? and + × −, the proportions were 91.4 per cent and 91.7 per cent. If all the progeny except those having positive parents are combined, positive diagnoses were recorded in 93.0 per cent. Doubtful cases were observed in 15.5 per cent and 16.9 per cent respectively.

As additional evidence of the genetic homozygosity of the mice of strain C58 MacDowell and Richter noticed that doubtful as well as non-leukemic offspring were scattered throughout the pedigree; not confined to one or more sublines. On the average non-leukemic individuals lived 100 days longer than animals which gave positive diagnoses.

The progeny of non-tumorous mothers of the C_3H or Z and the A strains have been tabulated in Table 2 to show the proportion of which developed mammary carcinoma. Both stocks have a high tumor incidence. In two classes the mothers had been mated to males from low-tumor stocks thus giving the incidence in the first generation hybrids. In another instance the females of the A strain which showed primary lung carcinoma are considered.

The total number of progeny observed by the author from non-breast tumor mothers was 94 and the mammary tumor incidence among them was 74.5 per cent. Of the 23 hybrid animals which had non-tumorous mothers from the high cancer strain 19 or 82.6 per cent developed breast tumors. Likewise the proportion of mammary tumors (70.6 per cent) among the progeny from mothers which had primary lung tumors is far greater than one would expect as due to chance alone.

TABLE 1

A classification of the offspring in strain C58 according to the presence or absence of leukemia in the parents

(Reproduced from MacDowell and Richter (1935), Table 4.)

PARENTS	OFFSPRING +	OFFSPRING −	OFFSPRING ?	PERCENTAGE OF DEFINITE DIAGNOSES POSITIVE FOR LEUKEMIA
+ × +	286	36	59	88.8
+ × ?	53	5	13	91.4
? × ?	11	0	1	100.0
+ × −	55	5	12	91.7
− × ?	4	0	0	100.0
− × −	10	0	3	100.0

These observations by Strong, MacDowell, Richter and Bittner show the value of using inbred strains of mice in cancer investigations and that the genetic constitution of all individuals—cancer as well as non-cancerous—within a pure stock is probably identical. Thus, animals which do not show cancer somatically in a high tumor strain of mice are potentially cancerous and must be considered as genetically cancerous. In mating these non-cancerous individuals all the evidence procured thus far from pure stocks demonstrates that they must be considered as cancerous parents.

Once inbred stocks have been established the next probable advance may be attained by outcrossing strains which differ markedly in their tumor incidence. That is, for example, a strain having a high incidence of breast cancer should be crossed to a race having little or no breast cancer. To maintain an absolutely non-breast tumor strain of mice is almost an impossibility and may never be attained. Several races have been observed after they had been inbred for the required number of generations to make the individuals homogeneous, for as long as ten years without the appearance of breast cancer, only to have one or two turn up within a short interval. Following the unexplained observation of these sporadic tumors no growths were recorded for another long interval. Whether the few tumors which were observed resulted from a proc-

TABLE 2

The proportion of the progeny from non-breast tumor mothers of the A and Z stock developing mammary tumors

STOCK	DIAGNOSIS OF MOTHERS	TYPE OF PROGENY	NO. OF PROGENY	PER CENT WITH BREAST CANCER
Z	Non-tumor	Z Stock	23	65.2
Z	Non-tumor	ZIF_1	17	88.2
A	Non-tumor	A Stock	31	77.4
A	Non-tumor	AXF_1	6	66.7
A	Lung tumor	A Stock	17	70.6
TOTAL			94	74.5

ess analogous to somatic mutation is problematical. Data tabulated for such a strain for certain periods might indicate that it was a non-tumorous stock.

In considering any experiment the above principles of genetics will be applied if the results are given a genetic interpretation.

In any review of the literature on cancer the work of Loeb, Tyzzer, J. A. Murray, and Lathrop and Loeb should be mentioned. Their work was conducted in the period during which genetics was in its infancy. Although inbred strains of experimental animals were not procured, their investigations furnished the basis for further experimentation and indicated that cancer susceptibility was inherited.

Another interesting experiment was published by Tureen and Loeb (1929). The degree of inbreeding of their strains of mice was not mentioned. Fourteen groups of hybrids were raised and were observed for tumor rate and tumor age. When the parents differed in these factors, the hybrids were somewhat intermediate in both tumor age and incidence; in other crosses it approached the age and rate of one parent more closely than the other. Either the female or the male parent might predominate.

Marsh (1929) also noted mammary tumors in the hybrids between high and low tumor stocks which had not been inbred. Likewise, the strains of mice used by Dobrovolskaia-Zavadskaia (1933) were not homogeneous.

A vast amount of work has been published by Miss Slye. Her theory as to the inheritance of cancer may best be expressed by a quotation from one of her latest publications (1933, p. 538):

> From consistent results obtained in this laboratory during twenty-three years and involving 116,000 autopsies, the general trend indicates two facts clearly:
>
> 1. The genetic difference between cancer susceptibility and cancer insusceptibility involved one gene; that is, these are unit characters.
>
> 2. In the stock in this laboratory, cancer susceptibility behaves like a recessive; insusceptibility like a dominant.
>
> There remains to be worked out (1) the problem of the interrelation of neoplasms of different types and of different locations, that is, what I shall call the "distributing factors"; and (2) the problem of the interrelation between the external factor, other internal factors, and the hereditary factors.

My definition regarding the use of analyzed stocks of mice in a genetic problem applies to this article. Quotations from Slye's reports will verify this statement:

"On Dec. 2, 1910 (over twenty-two years ago), the original cross was made which resulted in strain 73. The parents were a silver-faun female purchased from Abby Lathrop, fancier of Granby, Mass., and a piebald grey-white male of strain 90, classic in these studies. None of the ancestry of this female was on record. The female herself died without autopsy, but there was no external evidence of tumor.

"The parent male also died without autopsy. He too showed no evidence of tumor. His father, male 30, died with chronic nephritis without cancer. His mother, however, female 3, died of a mixed sarcoma-carcinoma of the mammary gland, malignant adenoma of the liver, and sarcoma secondaries in the kidneys from the sarcoma element of the mammary gland mixed tumor. Female 3 is also classic in these studies.

"The parent male of strain 73 was, therefore, certainly at least heterozygous to cancer. Sarcomas, carcinomas, adenomas, and leukemic diseases were frequent in strain 90. . . ." This extract is from the 32nd Report, p. 540 (1933).

The parent male of strain 73 was "a piebald grey-white male of strain 90" (1933). Another designation for this male parent is: "a Japanese Whitefoot male" (1914, p. 286, and 1931, Chart 1).

Female 3 and male 30 of strain 90 were the parents of the Japanese Whitefoot male used as the male parent of strain 73. In the 5th Report (1916, p. 495) we find that female 3 is a sister of male 30: "The significant progenitor here is female No. 3 of strain 90. Inbred with her brother, male No. 30. . . ." The 28th Report (1931, p. 1366) denotes that the ancestry of female No. 3 is unknown: ". . . .The ancestry behind female 3 is unknown to me, as she is the first member of the strain in my hands. . . ."

Thus, we find that the ancestry of the female parent of strain 73, a silver-faun female secured from Lathrop, is unknown. On the male parent side the ancestry is known for one generation. Needless to say the genetic or neoplastic constitution of the two parents of strain 73 was almost a mystery. It might be interesting to summarize Slye's interpretation.

In the 2nd Report (1914, p. 501) we find regarding the parents of strain 73:

"Both parents came of cancer families but both died before autopsies were held, and it is uncertain whether or not they would have developed cancer." Later in the 3rd Report (1915, p. 165) strain 90, from which the parent male of strain 73 was descended, is described as ... "a slightly tumorous strain of inbred grey-white piebald mice, carrying a low per cent of cancer." Another reference to strain 73 was given in the 16th Report (1921, p. 154): "Chart 9 shows part of strain 73, which was derived from the same female 3, mated this time with male 30. Male 30 came from a strain carrying tumors of the lung, mediastinum, and diaphragm, and was proved heterozygous to tumors of these organs, having been tested in various crosses." Strain 90 was also described in the 5th Report (1916, p. 482): "There have been in this strain malignant growths in the mammary gland, lung, liver, kidney, ovary, testicle and mediastinum."

Since the ancestry of strain 90 was unknown beyond female 3 and male 30, it is interesting to be advised regarding the heterozygosity of male 30 to various types of tumors, for, to my knowledge, no chart has been published by Slye showing that the descendants of this pair have been inbred for the required number of brother-to-sister matings to make the stock homogeneous. They were used, however, in hybrid crosses.

It might also be interesting to learn of the various types of cancer transmitted by female 3. In the 2nd Report (1914, p. 288) her condition was diagnosed thus:

"... Female No. 3, had sarcoma of the mammary gland with metastases in liver and kidney. ..." In the 5th Report (1916, Chart 5) female 3 was listed with "Sarcoma of the mammary gland and adenoma of the liver." Chart 7, same report, inserts the term "malignant" adenoma of the liver. The metastatic sarcoma of the liver and kidney was omitted.

By 1921 (16th Report, p. 141) the diagnosis for female 3 had become: "... The parent female, No. 3 had a sarcoma-carcinoma of the mammary gland, a malignant adenoma of the liver, and a metastatic sarcoma of the kidney. ..."

In this same report (p. 146) credit is given to this same mouse as follows:

"In these strains, then, female 3 introduced:
"a. Primary carcinoma of the mammary gland, ...
"b. Primary adenoma of the liver, ...
"c. Primary sarcoma of the liver, ...
"d. Primary sarcoma of the kidneys, ...
"e. Secondary sarcoma of the kidneys, ...
"f. Secondary sarcoma of the spleen, ...
"g. Primary sarcoma of the mesentery, ...
"h. Secondary sarcoma of the mesentery, ..."

Since female 3 introduced such a variety of neoplastic growths, according to Slye, it is evident that her neoplastic constitution would be uncertain until after several generations of her progeny had been raised. It would be difficult to determine definitely the types of tumors female 3 introduced into the hybrid crosses owing to the nature of the matings; many of the mice used, like female 3, came from stocks of unknown ancestry.

Little (1928) has shown that the recessive theory of Slye does not fit the data as published in her reports. I do not believe, however, that Little should have attempted to explain Slye's data for mammary gland cancer on the assumption that the homozygous type was lethal. It might have been advisable had he taken the view of Haldane who, in an article on "The Genetics of Cancer" (1933), in which Slye's work is not mentioned, stated:

Besides the work described above, a good deal has been done on stock which was not genetically homogeneous. From this work it is clear that, while a tendency to spontaneous cancer is hereditary, it is not due to a single gene, dominant or recessive, and also that a particular localization of cancer may be hereditary. ... But all work with genetically hetero-

geneous material is unsatisfactory, because any individual may die before reaching the cancer age, and no other individual will be of just the same genetical make-up. Hence really exact work is impossible.

It has been our experience in this laboratory that a large percentage of all tumors arising in mice are mammary gland carcinoma. They are very unusual in males. Slye (1926) objects to the use of cancer of the breast in tumor study because they are very uncommon in males. She further stated in 1928 (p. 83) that "Breast cancer susceptibility, therefore, cannot be sex-limited. . . ." As there are no limitations between male and female we may include her data on mammary gland carcinoma in any examination of her results.

I wish to tabulate briefly the progeny of a mating between two cancerous individuals from her reports. Before doing so I wish to make it clear that I have adopted the method used by Little (1928, p. 34) in determining the results:

"In these matings "non-cancerous" animals, both of whose parents were cancerous, can be fairly included. This is true because if cancer is a simple recessive, then all descendants of any two cancerous mice mated *inter se* are genetically cancerous regardless of their somatic appearance." That is, I have considered all the progeny of this cancer × cancer mating as being genetically cancerous and have tabulated them as being cancerous parents if they were mated, even though they did not develop cancer.

Miss Slye has objected to this procedure (1928, p. 84) in these words: "For if we assume . . . . that the offspring of double cancerous parentage must be potentially cancerous, and we count them as such, *we are already assuming what we are attempting to prove.*"

If we assume the single recessive factor "cc" to represent cancer susceptibility, according to Miss Slye's theory, a cancerous male or female, having the constitution "cc", mated to another cancerous male or female, also having the constitution "cc", would produce only progeny having, like their parents, the cancer susceptibility constitution "cc" in the F_1, F_2, ... F_n generation. Regardless of the somatic appearance of cancer or not, all the progeny are genetically cancerous and must be called cancerous parents if they are mated. They may not be called cancerous individuals without the diagnosis of cancer at autopsy but should produce only cancerous progeny. Recessive individuals are always homozygous (cancerous according to Slye's theory) and, according to the laws of genetics, must breed true.

In Chart 9, 7th Report (1916) Slye published the ancestry of strain 338.

This chart is reproduced in part (Chart 1). The parents of this strain were female 5417 and male 7736. In the ancestry we again find female 3 and male 30 mentioned above.

The parents of strain 338 are not descended directly from female 3 and male 30. If we consider mice 3 and 30 to belong to the first generation, female 5417 and male 7736 are representatives of the sixth generation. By brother-to-sister matings it would take five matings to obtain progeny of the sixth generation. To produce the parents of strain 338 thirteen matings were made, three of which were not brother-to-sister. In addition, No. 5417 and No. 7736 are representatives of different sub-lines making strain 338 an outcross between lines of heterozygous individuals.

This particular mating was selected as both parents were cancerous and produced a large number of offspring. Two of their progeny were also considered by Slye as producing a 100 per cent cancer strain of entirely different derivation, that is strain 338, branch V. This family was derived from a double cancerous parentage, female 8619 with two carcinomas of the mammary gland and male 8715 with an adenoma of the liver. . . ."

In this chart (Chart 9, 1916) female 5417 was reported to have had 2 carcinomas of the mammary gland, carcinoma of the pelvis and metastases in the lungs. The parent male 7736 died of a papilloma of the lung and arterial sclerosis. In Chart 14, 16th Report (1921) male 7736 was reported as having had an adenoma of the lung.

Strain 338 was thus derived from a double cancerous parentage. All the descendants of this mating were cancerous genetically, if not somatically, and should be counted as cancerous parents.

In Table 3 we have tabulated the descendants of female 5417 and male 7736 according to the matings of the F_1 progeny of this pair. The mice are grouped according to sex. The mammary gland

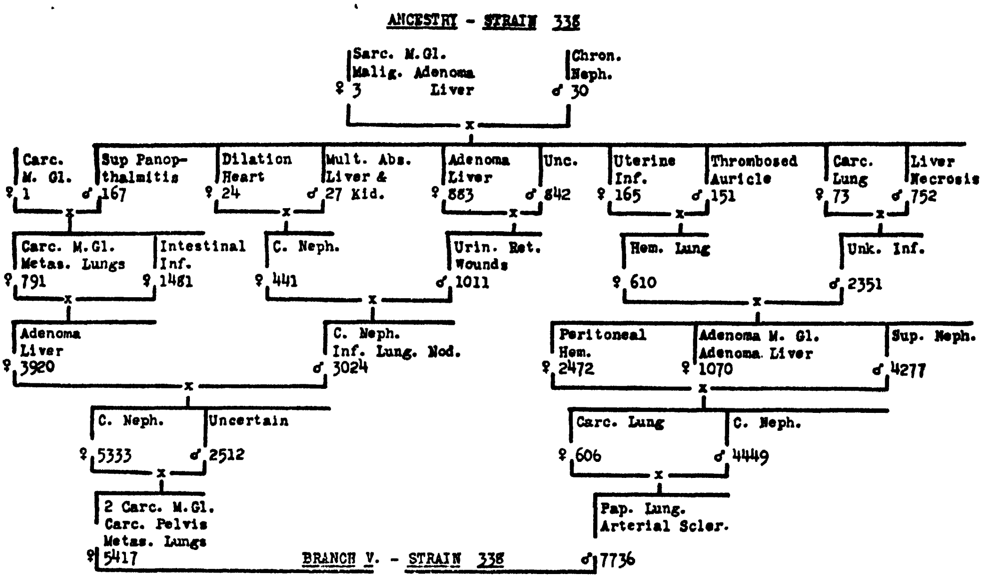

CHART 1. THE ANCESTRY OF ♀ 5417 AND ♂ 7736, THE PARENTS OF BRANCH V., OF SLYE'S STRAIN 338. (Taken from Syle: 7th Report, Part of Chart 9 (1916))

TABLE 3

The descendants of cancerous female 5417 (carc. m. gl., carc. pelvis, metas. lungs) mated to cancerous male 7736 (papilloma or adenoma lung)

(The figures are from the data published by Miss Slye)

F_1 MATING	CHART	REPT.	CARC. M.GL.		OTHER TUMORS		TOTAL CANCER			TOTAL NON-CANCER			CANCER
			♀	♂	♀	♂	♀	♂	Total	♀	♂	Total	*per cent*
6993 × 6851	11	9	6	0	2	1	8	1	9	4	8	12	42.9
7899 × 6851	9	9	8	0	2	2	10	2	12	8	8	16	42.9
8619 × 5815	12	9	6	0	2	2	8	2	10	6	5	11	47.6
8619 × 5815*	11	7	1	0	1	1	2	1	3	1	2	3	50.0
8619 × 7394**	14	16	3	0	0	2	3	2	5	0	0	0	100.0
8619 × 8751	14	21	10	0	4	8	14	8	22	0	0	0	100.0
8619 × 8751***	9	7	12	0	1	2	13	2	15	5	14	19	44.1
9544 × 6441	10	9	4	0	4	1	8	1	9	3	7	10	47.4
TOTAL			50	0	16	19	66	19	85	27	44	71	54.5

*Does not include progeny given in Chart 12, 9th Rept.

**Does not include progeny from ♂ 9783 × ♀ 11560

***Does not include progeny given in Chart 14, 21st Rept.

carcinomas are listed separately but all other types of tumors are grouped together.

Female 5417 and male 7736 had 156 descendants which we were able to tabulate. As they were cancerous individuals all of their descendants should have been cancerous. There were 93 females, 66 of which had neoplasms or 70.97 per cent ± 3.14. Of the 63 males 19 or 30.16 per cent ± 3.99 had cancers. The difference between the sexes was 40.81 per cent ± 5.08 or 8.03 × P.E. The total number of individuals gave 54.49 per cent ± 2.66 cancerous mice. The difference between this observation and the expectation of 100 per cent tumorous, according to the recessive theory, was 45.51 per cent ± 2.66 or 17.10 × P.E.

For the benefit of the readers not familiar with probable error determinations, I might add that a difference greater than 4 × P.E. (1:142) denotes a significant variation from expectation in genetic work. The odds against a deviation of 5 × P.E. being due to chance are 1 to 1,341. Or expressed in terms of our ratio 8.03 × P.E. for the sexes, we should expect such a deviation, if Miss Slye's theory is correct, once in approximately 15 million times.

One will note from the table that two of the matings gave 100 per cent cancerous progeny. Chart 14, 16th Report (1921), giving the mating of 8618 × 7394, showed that other progeny were observed but not listed. The other mating was 8619 × 8751. In Chart 14, 21st Report (1926) all the progeny listed had neoplasms. In 1916 (Chart 9, 7th Report), ten years previous, other progeny are included, many of which are non-tumorous animals. The sex difference for the young descended from female 8619 and male 8751 was 5.34 × P.E., and the degree of significance for the entire group was 8.04 × P.E. when compared with the 100 per cent cancerous expectation. Thus, we observe that instead of producing 100 per cent cancerous progeny so few produced tumors that a significant difference was observed between the observed data and the expectation. While one does not expect 100 per cent cancer in any line, comparisons were made with this figure as Miss Slye stated that such were the data obtained.

If we exclude the carcinomas of the mammary gland, observed only in females, there is no significant difference between the sexes of the mice descended from female 5417 and male 7736. Since this type of cancer represented 76 per cent of all classes of tumor occurring in the females and 59 per cent of all tumors for the combined sexes, there is no apparent reason for not tabulating them. Rather it might be more important to consider a cross where only this type of cancer was observed.

TABLE 4

Types of matings of the first generation progeny of the cancerous parents 5417 and 7736 from Miss Slye's data

(References are given in Table 3.)

MATINGS	CANCER	NON-CA.	CANCER (per cent)
Cancer 6993 × Non-ca. 6851	8	11	42.1
Non-ca. 7899 × Non-ca. 6851	12	14	46.2
Cancer 8619 × Non-ca. 5815	12	13	48.0
Cancer 8619 × Cancer 7394	5	0	100.0
Cancer 8619 × Cancer 8751	35	19	64.8
Cancer 9544 × Non-ca. 6441	8	9	47.1

The data may also be considered according to the diagnosis of the progeny of female 5417 and male 7736 as has been done in Table 4.

Six matings were made. Although the parents were both cancerous not all of the first generation progeny were such. Two cancerous × cancerous matings gave 64.8

per cent and 100 per cent cancerous descendants. The latter mating gives only five progeny; Chart 14, 16th Report (1921) shows that some are not listed. The three cancer × non-cancer matings gave 42.1 per cent, 48.0 per cent, and 47.1 per cent cancer offspring. The single non-cancer × non-cancer cross gave 46.2 per cent cancer. One non-cancerous male, 6851, was mated twice to cancer female 6993 and non-cancer female 7899. The proportion of the descendants which were cancerous in the two matings were 42.1 per cent and 46.2 per cent respectively.

Cancer female 8619 was mated to three of her brothers. By non-cancer male 5815 she produced 48.0 per cent cancer, by cancer male 7394 all listed progeny were cancerous, and by cancer male 8751 64.8 per cent were cancerous.

The mathematical differences between Slye's results and the expectation of 100 per cent cancerous expectation might be explained by assuming that the non-cancerous mice died before reaching the cancer age. Granting as much, the recessive theory is still unable to explain the sex difference which was statistically significant as the theory states that mammary tumors are not sex-limited.

We may conclude that these data of Slye involving 156 descendants from cancerous parents do not support the theory that cancer susceptibility is dependent on a unit recessive factor. Since this article was written Miss Slye has altered her theory of cancer (Occasional Pub. of A. A. A. S., No. 4, 1937).

The status of the controversy has been ably summed up by Wells (1931):

> Obviously the genetic mechanism of the inheritance of susceptibility to cancer is still an unsettled problem, but data are now being accumulated in many places which should soon clear up many of the now contradictory contributions. . . . At the present time it seems safe to maintain that the existence of an hereditary influence on the susceptibility and resistance to cancer has been established both for man and animals. The exact mechanism of the hereditary influence has yet to be determined. . . .

In considering the genetic work on the inheritance of cancer it is evident that not all types follow the same principles of heredity. As most of the problems are on mammary gland, lung or leukemia, it seems advisable to tabulate each type separately.

MAMMARY GLAND TUMORS

Before reviewing the results on the inheritance of the susceptibility to breast cancer, a list of the inbred stocks of mice used will be given. Practically all matings were brother-to-sister or the progeny mated back to one of their parents.

High tumor strains

Dilute brown dba stock (Murray-Little). Inbred since 1909 or for more than 75 generations (Murray, 1934). More than 75 per cent of the breeding females living four months or longer develop carcinoma of the breast. Murray and Little (1935a) found that the incidence in virgin females was 50.8 per cent.

"A" Stock. A strain of albino mice started in 1921 by Strong (1936a). Fifty generations have been obtained by brother-to sister matings. From unpublished data the breast tumor incidence in breeding females was found to be 82.4 per cent (also Bittner, 1935c).

C3H or "Z" Stock. Inbred since 1920 or more than 35 generations (Strong, 1935a and b). Of the breeding females living five months or longer 78 per cent develop mammary cancer (Bittner, 1935b).

"D" Stock (Strong). A sub-line of the dba strain. The incidence in breeding females was 56.3 per cent, average age 14.2 months (Bittner, 1936a).

Low tumor lines

C57 Black Stock. A strain of black mice inbred by Little since 1921. No breast cancers were observed among 240 breeding females by Murray and Little (1935a). Since that publication two have been recorded among several hundred additional breeding mice.

CBA or "X" Stock. Inbred since 1920 by Strong (1936b) who reported 2 tumors among 71 breeding females. In the author's line 13.5 per cent of 125

females have developed breast tumors, average age 21 months.

"N" *Stock*. A line obtained from Strong which has been inbred for 20 generations. The breast tumor incidence among 92 breeding females was 10 per cent, average age 17 months.

Mus bactrianus. Inbred since 1927. The tumor incidence in breeding females was about one per cent.

Yellow Stock. A line having a low incidence of breast tumors but a relatively high proportion of internal tumors (Little, 1934). Inbred since 1927.

"I" *Stock*. A race obtained from Strong in which no breast tumors had been recorded for twelve generations in a small number of progeny.

Zavadskaia brachyuric Stock. Representatives of this line were received from Dr. L. C. Dunn in 1931. This strain has not been closely inbred. Cloudman and Little (1936) observed one breast tumor in 52 virgin females.

Little in 1934 reported on the cross between the dilute brown stock and the yellow strain. Females from the high tumor line were used in making the first generation hybrids. The reciprocal cross was not made. The tumor incidence in hybrids according to the color of the mice was:

	No.	Mammary tumors (per cent)
F_1 Generation		
Yellow mice	57	38.6
Non-yellow mice	54	64.8
Total	111	51.4
F_2 Generation		
Yellow mice	108	42.6
Dilute yellow mice	48	25 0
Brown mice	156	53.8
Dilute brown mice	67	46.3
Total	379	45 6
F_2 Generation		
All yellow mice	156	37.2
All non-yellow mice	223	51.6

Little found in the first and second hybrid generations that the yellow animals had fewer tumors than the non-yellow, the most probable explanation being the metabolic or physiological differences. Another important conclusion was that in a study of either the genetics or physiology of the incidence of spontaneous tumors, not all types of tumors or all colors of mice can be considered together.

A preliminary report of several crosses between high and low mammary tumor strains was published by the Staff of the Jackson Laboratory in 1933. In considering these observations reference will also be made to later publications where possible.

In these experiments when the maternal parents came from inbred high breast cancer lines, a high incidence of mammary cancer was also noted in their female descendants of the hybrid generations. In crosses where the maternal parents came from a low tumor stock the incidence of mammary tumors was significantly less in their female hybrid descendants. The chromosomal contents of the reciprocal hybrid mice were theoretically the same although the extra-chromosomal contents varied according to the maternal parent used in making the cross. In this preliminary report the theory of extra-chromosomal influence was advanced for the inheritance of susceptibility to mammary gland tumors, at least in mice.

Experiments supporting this theory are listed below:

dba × C_{57}Blk. Cross (Staff Jackson Lab., 1933, Murray and Little, 1935a).

Reciprocal crosses between the dilute brown high tumor and the C_{57}Black low tumor lines gave the following results in hybrid generations. Virgin females only were tabulated.

	No.	Mammary cancer (per cent)
dba ♀ × C_{57}Bl♂ = dBF$_1$	113	39.82
C_{57}Bl ♀ × dba♂ = BdF$_1$	356	6.05
dBF$_1$ ♀ × dBF$_1$♂ = dBF$_2$	664	35.54
BdF$_1$ ♀ × BdF$_1$♂ = BdF$_2$	687	5.96

These results have been verified by Korteweg (1934). Preliminary work by Murray and Little (1935b, 1936) has resulted in the following data for the back-cross generations. The approximate number in each class is 125 animals.

	Mammary cancer (per cent)	Descended from
BdF$_1$ ♀ × dba♂	7.14	Black mother
dba ♀ × BdF$_1$♂	53.47	Dilute brown mother
BdF$_1$ ♀ × C_{57}Bl♂	6.00	Black mother
dBF$_1$ ♀ × C_{57}Bl♂	50.86	Dilute brown mother

dba High Tumor × *Mus bactrianus* Low Tumor (Staff Jackson Lab., 1933)

	No.	Mammary cancer (per cent)
dba ♀ × *M. bact.* ♂	69	68.11
M. bact. ♀ × dba ♂	27	7.41

"A" High Tumor × CBA or "X" Low Tumor (Staff Jackson Lab., 1933, Bittner, unpublished).

The incidence in this and the following crosses was for breeding F_1 females.

	No.	Mammary cancer (per cent)
A ♀ × X ♂ = AXF_1 breeding ♀	44	90.9
X ♀ × A ♂ = XAF_1 breeding ♀	16	18.8

C_3H or "Z" High Tumor × "I" Low Tumor (Staff Jackson Lab., 1933, Bittner, In Press).

	No.	Mammary cancer (per cent)
Z ♀ × I ♂ = ZIF_1 breeding ♀	36	91.7
I ♀ × Z ♂ = IZF_1 breeding ♀	10	0.0

C_3H or "Z" High Tumor × "N" Low Tumor (Bittner, unpublished).

	No.	Mammary cancer (per cent)
Z ♀ × N ♂ = ZNF_1 breeding ♀	48	97.9
N ♀ × Z ♂ = NZF_1 breeding ♀	18	27.8

"A" High Tumor × "D" High Tumor (Bittner, 1936).

	No.	Mammary cancer (per cent)	Aver. age Ca. (in months)
"A" Stock breeding ♀♀	421	63.9	12.3±0.13
A ♀ × D ♂ = ADF_1 breeding ♀♀	403	86.5	12.1±0.20
"D" Stock breeding ♀♀	207	56.3	14.2±0.20
D ♀ × A ♂ = DAF_1 breeding ♀♀	67	71.9	15.6±0.39

dba High Tumor × Zavadskaia brachyuric Low Tumor (Cloudman and Little, 1936).

In this outcross dilute brown females were mated to brachyuric males. Cloudman and Little observed the tumor incidence in virgin F_1 and back-cross females. The latter generation was made by mating F_1 males to C_{57} Black females.

	No.	Mammary cancer (per cent)
dba ♀ × Zav. bra. ♂ = dZF_1	163	76.7
dZF_1 ♂ × $C_{57}Bl$ ♀ = BC	212	5.66

The experiments mentioned above are tabulated in Table 5. The incidence of mammary tumors in the F_1 generation females only is given.

SPONTANEOUS LEUKEMIA

Work on the inheritance of leukemia in mice has been reported by Slye (1931); MacDowell and Richter (1935), MacDowell (1935), and Richter and MacDowell (1935).

In the latter article we find regarding the work of Slye:

Slye observed cases of mouse leukemia only in "tumor strains." She regards leukemia as a neoplasm, and believes that the genetic basis of all neoplasia is a recessive Mendelian gene.

Three objections may be raised to Slye's conclusions.

1. The paper on leukemia presents only a portion of the cases observed (44 of 975). Complete data are necessary in this instance, for the conclusions are based on the *ex post facto* interpretation of pedigrees, rather than on experimental tests.

2. The hypothesis has not been tested by crosses between genetically homogeneous strains, as required for a genetic conclusion.

3. Genetic ratios are supposed to be indicated by the somatic condition of the mice—i.e. leukemic or non-leukemic. In view of the results obtained by MacDowell and Richter, mentioned below, the occurrence of leukemia does not necessarily indicate the genetic condition of the animal. For the determination of genetic ratios, breeding tests of individuals in a segregating generation are necessary.

The strains of mice employed by MacDowell and Richter in their leukemic studies were known as the C58 and the Storrs-Little stocks. Strain C58 has been inbred since 1920 or for over 25 generations by brother-to-sister matings. Approximately 90 per cent of the animals develop spontaneous leukemia of the various types. The Storrs-Little line has also been inbred for over 25 generations. The incidence of leukemia in this line was roughly one per cent.

The data resulting from the hybrid crosses conducted by MacDowell and Richter (1935) are listed on the following page.

TABLE 5

Various crosses between high mammary cancer and low cancer strains made by the members of the staff of the Jackson Memorial Laboratory

(Reference to each cross is given in the text. The incidence of mammary cancer in F_1 hybrid females is given in the last two columns.)

STOCKS CROSSED		HIGH CANCER STOCK			LOW CANCER STOCK		TYPE F_1 ♀	HIGH Ca ♀ × LOW Ca ♂		LOW Ca ♀ × HIGH Ca ♂	
High Ca	Low Ca	Gener. inbred	Ca. incidence Breed.	Ca. incidence Virg.	Gener. inbred	Cancer breeders		No.	Ca	No.	Ca
			per cent	*per cent*		*per cent*			*per cent*		*per cent*
dba	× Yellow	75+	80	51	15	10	Virg.	111	51.4	—	—
dba	× C_{57}Blk	75+	80	51	40	−1	Virg.	113	39.8	356	6.1
dba	× *M. bact.*	75+	80	51	20	1	Virg.	69	68.1	27	7.4
dba	× Brachyuric	75+	80	51	10	5	Virg.	163	76.7	—	—
"A"	× "X"	50	82	5	30	14	Breed.	44	90.9	16	18.8
"Z"	× "I"	30	78	?	12	?	Breed.	36	91.7	10	0.0
"Z"	× "N"	30	78	?	20	10	Breed.	48	97.9	18	27.8

	No.	Leukemic (per cent)
C58 ♀ × Sto.-Li.♂ = F_1♀	86	56.2
C58 ♀ × Sto.-Li. ♂ = F_1♂.....	80	68.2
Total.....................	166	61.9
Sto.-Li ♀ × C58♂ = F_1♀	77	40.6
Sto.-Li. ♀ × C58♂ = F_1♂.....	63	45.2
Total	140	42.5

The difference between the incidence in the reciprocal hybrids was 19.4 per cent ±4.3 or 4.5 × P.E. In addition the hybrid animals which had mothers from the Storrs-Little stock lived 138 days longer than the other group.

The back-cross data to the Storrs-Little stock follows:

	No.	Leukemia (per cent)
F_1 ♂ (C58♀ × Sto.-Li.♂) × Sto.-Li.♂..................	114	19.8
F_1♀ (C58♀ × Sto.-Li.♂) × Sto.-Li.♂..................	188	46.5

The degree of significance between the results of the cross was 7.0 × P.E.

From these data MacDowell and Richter advanced the theory for the non-chromosomal maternal influence in the etiology of leukemic diseases in mice.

LUNG CARCINOMA

Tyzzer (1907, 1909) observed a larger proportion of lung tumors among the offspring of tumorous parents than tumor-free parents.

According to the recessive theory of Slye lung carcinoma would be inherited as a Mendelian recessive character.

Several reports on the inheritance to susceptibility to pulmonary tumors have been published by Lynch (1926, 1928 and 1931). The inbred low tumor strain 1194 was crossed to strain D having an incidence of approximately 40 per cent. The latter strain had been pen inbred and was, therefore, not very homogeneous. Females from strain 1194 were crossed to males from the D stock (1931). The proportion of F_1 hybrids living six months or longer which developed lung tumors was 24.4 per cent. First generation hybrid animals were back-crossed to individuals of the low tumor line 1194 and the resulting generation animals had a tumor incidence of 7.3 per cent. In the back-cross generation to the D strain the proportion of tumors was 32.2 per cent.

Lynch concluded that susceptibility to lung tumors was dominant or semidominant; the somatic development of tumors in animals which were genetically "tumor mice" was evidently prevented because of age and other influences.

DISCUSSION

From the work mentioned above it is evident that not all workers on the genetics of cancer susceptibility are securing similar results. The difference, in general, may be accounted for by the different methods of approach to the problem and the various genetic constitutions of the material used.

In the chemical preparation of a substance the chemist refuses to use chemicals which he knows in advance are not pure.

In the analysis of a genetic problem the investigator should consider the purity or the homozygosity of his material. To go further, not all chemicals which are white are the same compounds; not all mice which are white have the same genetic constitution and, unless they come from the same inbred stock, should not be considered as identical. This point is clearly brought out in the study of transplantable tumors by Tyzzer, Little, Strong and others. The use of animals produced at random—"market mice"—produced variations which could not be explained or duplicated. Mice of known ancestry from a single inbred strain, on the other hand, have given uniform results over a period of years.

Geneticists using inbred strains of mice have observed that the offspring from non-tumorous mothers of a high cancer line have as high a spontaneous tumor incidence as do progeny from tumorous mothers of the same race. Just as all the samples of a pure chemical are the same, so are all the individuals of a pure strain of mice identical.

The value of such strains for use in outcrossing is beyond question. The consistency of the results obtained by the use of various strains or different experimenters using the same material leaves little doubt regarding the significance of the observations. Although one worker states that the susceptibility of all types of neoplasia is inherited in the same manner, the work of others with more suitable material throws doubt on such a simple explanation. The cancer problem would probably be much simpler if all cancers had the same inherited susceptibility.

Seven outcrosses have been made by the members of the Staff of the Jackson Memorial Laboratory between high and low mammary tumor lines. Reciprocal F_1 generations were obtained in five of the crosses. In every cross mammary tumors were observed in the first generation hybrid females, either virgins or breeders. In every reciprocal cross the incidence of mammary tumors in the hybrid females was much higher when the mothers were from the high tumor line. To explain such results the extra-chromosomal hypothesis has been advanced. Additional evidence for such an explanation has been obtained by Murray and Little (1936) by controlled back-cross matings between the dBa and the C_{57}Blk strains. Korteweg (1934) has confirmed the evidence observed in the hybrid generations by using the same strains.

The work of MacDowell and Richter (1935) also shows that a non-chromosomal influence is involved in the inheritance of spontaneous leukemia in mice. The proportion of the offspring having this type of cancer was again higher when the mothers were from the high cancer line. The degree of significance was not as great, however, as in the above experiments which considered only mammary cancer.

In Lynch's experiments lung tumor susceptibility was observed to be transmitted through the male parent. The reciprocal cross was not made. A small number of animals observed by the author shows that it may be inherited through either parent.

Although susceptibility to mammary cancer and leukemia apparently follow the same type of extra- or non-chromosomal inheritance, one striking difference has been noted. That is, the incidence of leukemia in males approximates that of females. The chances of a male mouse having a mammary tumor is negligible.

Another distinction between these two forms of neoplasia may also be mentioned. MacDowell and Richter (1935) found that when the young from leukemic

parents were fostered by nursing mothers from a non-leukemic stock there was no change in the incidence among the fostered progeny. When young from mothers of the "A" high mammary strain are raised by mothers from low tumor lines the incidence of breast tumors in the fostered females, which were used as breeders, was significantly smaller than in the unfostered series of the same stock (Bittner, 1936b). This influence was transmitted to subsequent generations. Further work indicates that a "breast cancer-producing influence" may be transmitted in the milk of breast cancer mothers, offering an explanation for the extra-chromosomal theory of breast cancer. This work has been confirmed by Little using transplanted fertilized ova (Bittner and Little, J. Heredity, 28: 117, 1937) and by Andervont (unpublished) by the foster-nursing of the progeny from cancer mothers by low cancer stock females.

Primary lung cancer may occur in animals of either sex. No evidence has as yet been accumulated to date that it is extra-chromosomal in transmission.

From the data tabulated during the past ten years it is evident that some progress has been made in the field of cancer research in comprehending some of the details of the inheritance of susceptibility to some forms. Not all types of tumors may be grouped and studied in a single experiment; each type must be considered by itself. The problem is by no means solved but at least a start has been made by the use of homozygous stocks of mice and increasing interest in the problem by geneticists. Much of the controversy is due to the application of genetic principles to data not suitable for interpretation in this field.

The application of the findings in animal experimentation work to the human problem is problematical and remains for the future. The impossibility of securing human data to compare with inbred strains of animals where controlled matings may be made needs little comment. The reliability of considerable of the human data is questionable and leaves much to be desired.

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THE NUMBER AND MENDELIAN RATIOS OF PHENOTYPES AND GENOTYPES

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THE purpose of this paper is to offer a few convenient tables presenting the numbers and ratios of phenotypes and genotypes for any degree of hybridism. Although the results obtained are not new, the mode of presentation and the simple rules for deriving figures for any degree are, as far as the authors know, novel. Most textbooks on genetics, and many technical papers, treat only of low degrees of hybridism. Their method of presentation consists chiefly of square graphs (Punnett squares) or of configurations in the form of family trees, neither of which are convenient for determining high degrees. Exceptions to this approach will be mentioned in the body of this paper.

We therefore consider it desirable to publish exhaustive tables which designate phenotypic and genotypic ratios for any degree of hybridism. These tables furthermore correlate the results in a systematic manner and thus provide other information not readily seen in previously published expositions on this topic. The first two tables deal with phenotypes; the final table combines the mathematical relationships of phenotypes and genotypes. The accompanying explanation of the tables includes mathematical verifications of the relationships involved in the tables.

PHENOTYPES

In Table 1 the first row represents the number and ratio of monohybrids; the second row the number and ratio of dihybrids; and so forth. The numbers within the parentheses are the number of individuals displaying a given set of characters; the numbers outside the parentheses designate the number of types having the membership stated within the parentheses, and hence are coefficients of the numbers within the parentheses. The first column from the left represents the number of individuals displaying only dominant characters. Hence for every degree of hybridism there is only one such type. The second column represents the number of individuals displaying one recessive character and, by means of the coefficients, the number of types in a given degree of hybridism which display only one recessive character. The third column represents two recessive characters, and so on to the last column for each degree, where are to be found the number of individuals displaying only recessive characters. Therefore the coefficients on the uppermost diagonal are all 1. The column on the extreme right gives the theoretical total number of individuals.

To illustrate, let us consider row 4, which represents the tetrahybrids. The Mendelian ratio for this degree is

$$81:27:27:27:27:9:9:9:9:9:9:3:3:3:3:1$$

Theoretically, 81 individuals will show only dominant characters; four groups of 27 each will show one recessive and three dominant characters; six groups of 9 each will show two recessive and two dominant characters; four groups of 3 each will

show three recessive and one dominant character; finally one unit group shows all recessive characters. The theoretical total number of tetrahybrids is 256.

It is of interest that any row of this table can be analyzed by means of the technique evolved by Symbolic Logic in its algebra of classes. Each dominant character can be thought of as determining a class. Then the total number of dominant and recessive characters (= non-dominant) will be the total number of subclasses determined by the degree of hybridism; i.e. by the number of dominant characters being considered. It has been established that the total number of subclasses is 2^n, where n is the number of classes. In other words, we wish to make a full expansion of the universe defined by the number of dominant characters. Again the case of the tetrahybrid is used for illustration.

Let A, B, C, D be the four dominant characters and a, b, c, d the corresponding recessive characters. The following list exhausts all the possible combinations of male gametes resulting from the union of dissimilar parents. An identical list exhausts all the possible female gametes.

ABCD	aBCD	AbcD	aBcd
ABCd	ABcd	aBcD	abCd
ABcD	AbCd	abCD	abcD
AbCD	aBCd	Abcd	abcd

By mating each type of sperm to every type of ovum, one gets $16 \times 16 = 256$ types of the F2 generation. From this list, the characteristics of mating each sperm to every ovum can be immediately determined, remembering that

$$Y \times Y = Y, Y \times y = Y, y \times y = y$$

It is immediately apparent that there will be exactly

1 case of only capital letters;
4 cases of exactly one lower case letter: a, b, c, d;
6 cases of exactly two lower case letters: ab, ac, ad, bc, bd, cd;
4 cases of exactly three lower case letters: abc, abd, acd, bcd;
1 case of exactly four lower case letters: abcd.

The number of cases are the coefficients of the fourth row of Table 1. Thus this method, which can easily be generalized for any number of characters, constitutes an analysis of each row.

The beautiful symmetry displayed by the table is worthy of note, for it permits one to extend it to any degree. The numbers within the parentheses of each column and their coefficients are terms of geometrical and arithmetical series respectively. The common ratio of the geometrical series is 3. The series of coefficients are arithmetical progressions. Thus, for instance, the coefficients of the fourth column: 1, 4, 10, 20, 35, 56, . . . can be reduced to 3, 6, 10, 15, 21, . . . (the coefficients of column 3), which in turn is reduced to 3, 4, 5, 6, . . . Interesting relationships can also be found by reading the table diagonally.

The following simple analysis is a further clarification and proof of the accuracy of the arithmetical progressions. The coefficients of column 2 represent the number of terms in a full expansion involving one recessive character. By a *full expansion* is meant here a list of terms such that every term contains each dominant or corresponding recessive character. The preceding list was a full expansion of the entire domain of four dominant and recessive characters. Column 2 is limited to one recessive character. Thus in the case of dihybrids we have Ab and aB;

trihybrids, ABc, AbC, aBC; etc. In short, there are two arrangements of one dominant and one recessive character, three arrangements of two dominant and one recessive, four arrangements of three dominant and one recessive character. In general, letting d be the degree, we have the general formula

$$C^d_1 = d$$

which expresses the arithmetical progression of column 2; i.e. the combination of d things taken 1 at a time. Likewise the progression of coefficients in column 3 is a series of full expansions of terms with two recessive characters. The general formula will be

$$C^d_2$$

i.e. the combination of d things taken 2 at a time. The formula for column 4 is

$$C^d_3$$

In general, the coefficient of any term in column 2 and onward will be

$$C^d_r$$

where r is the number of recessive characters.

The *number* of phenotypes for any degree of hybridism is found by adding the coefficients of the row presenting the degree. However, it is clear that this number will be identical with the number of male gametes or of female gametes. Therefore the number of phenotypes will be

$$2^d$$

where d is the degree of hybridism. Note that $d = n$, where n is the number of dominant characters. This result also follows from the logical expansion given above in that the number of phenotypes is the square root of the total number of individuals of a degree. Example: for tetrahybrids there are $2^4 = 256^{1/2} = 1 + 4 + 6 + 4 + 1$ phenotypes.

Bateson (3), Babcock and Clausen (2), and others have pointed out that the phenotypic ratio for any degree is an expansion of the binomial $(3 + 1)^n$, where n is the degree. Thus again to use the tetrahybrid as an example,

$$256 = (3 + 1)^4 = 1 \times 3^4 + 4 \times 3^3 + 6 \times 3^2 + 4 \times 3 + 1 \times 1.$$

In general [See Bateson, *op. cit.*, page 59; also Pearson (9), page 56, for determination of 4^n.]

TABLE 1

Phenotypes: increasing recessives

$(3)^1$	$(1)^1$								4
$(9)^1$	$(3)^2$	$(1)^1$							16
$(27)^1$	$(9)^3$	$(3)^3$	$(1)^1$						64
$(81)^1$	$(27)^4$	$(9)^6$	$(3)^4$	$(1)^1$					256
$(243)^1$	$(81)^5$	$(27)^{10}$	$(9)^{10}$	$(3)^5$	$(1)^1$				1024
$(729)^1$	$(243)^6$	$(81)^{15}$	$(27)^{20}$	$(9)^{15}$	$(3)^6$	$(1)^1$			4096
$(2187)^1$	$(729)^7$	$(243)^{21}$	$(81)^{35}$	$(27)^{35}$	$(9)^{21}$	$(3)^7$	$(1)^1$		16384
$(6561)^1$	$(2187)^8$	$(729)^{28}$	$(243)^{56}$	$(81)^{70}$	$(27)^{56}$	$(9)^{28}$	$(3)^8$	$(1)^1$	65536
.	.	.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.	.	.

$$
\begin{aligned}
4^n = 3^n & \\
& + 3^{n-1} + 3^{n-1} + \cdots n \text{ times} \\
& + 3^{n-2} + 3^{n-2} \\
& + \cdots \frac{1}{2} n(n-1) \text{ times} \\
& + 3^{n-3} + 3^{n-3} \\
& + \cdots \frac{1}{6} n(n-1)(n-2) \text{ times} \\
& \vdots
\end{aligned}
$$

This somewhat obscure statement of the general formula can be clarified by rewriting it as follows, remembering that one of the terms of the binomial is unity.

$$
4^n = 3^n + n3^{n-1} + \frac{n(n-1)}{1 \cdot 2} 3^{n-2} + \frac{n(n-1)(n-2)}{1 \cdot 2 \cdot 3} 3^{n-3} + \cdots
$$

This ingenious mathematical application gives the numbers in each row of our Table 1. However, the formula must be applied with care for we are dealing not with numbers but with phenotypes. Thus it will be observed that Bateson's third line in the original is inaccurate, for the ratio is

$$27:9:9:9:3:3:3:1$$

and not, as is given there

$$27:27:9:1.$$

The application of the propositions of pure mathematics is often obscure, whereas our tables prevent the emergence of such inaccuracies.

When the formula is interpreted phenotypically, it is of considerable value. Let the exponent of the binomial be the degree of hybridism and let the coefficients in the expansion represent the number of times the corresponding terms are repeated in the ratio. These coefficients correspond to our numbers outside of the parentheses. The addition signs have no significance phenotypically. The sum of all the terms in the expansion is the total number of individuals. It must be remembered that multiplication takes precedence over addition. The coefficients are always integers, for the numerator of each coefficient will be exactly divisible by its denominator.

Pearson's analogous statement of this expansion, in his already cited paper, is in some respects preferable to the direct use of the binomial $(3 + 1)$ in that he shows the genetic constitution of each term. This paper is so important to the topic at hand that we consider it necessary to present a part of it. The union of heterozygotes aA and aA give rise to the combinations aa, 2aA, AA. Letting u = aa, v = aA, w = AA, the union of the corresponding couplets results in

$$u + 2v + w.$$

Hence the general distribution for degree n will be

$$
(u + 2v + w)^n = u^n + nu^{n-1}(2v + w) + \frac{n(n-1)}{1 \cdot 2} u^{n-2}(2v + w)^2 + \cdots
$$

Basically this is the same as the binomial given above, for it is an expansion of the multinomial $(1 + 2 + 1)^n$.

By "protogenic couplet" Pearson refers to the combination or zygote AA; "allogenic" designates the couplet aa; and "heterogenic" signifies the couplets Aa or aA. "Thus, for example, there would be out of a total population of possibilities 4^n: 1 purely allogenic individual, $n \times 3$ individuals with $n - 1$ allogenic couplets; $2n$ of these would have one heterogenic couplet, and n would have one protogenic couplet." [Pearson (10) p. 57; Blaring-

hem (4) p. 293.] For instance, among the dihybrids there are two individuals with one protogenic couplet each, namely aa*BB* and *AA*bb; and four individuals with one heterogenic couplet each: aa*bB*, aa*Bb*, *aA*bb, and *Aa*bb. Each of these six individuals has one allogenic couplet. It can now be readily seen that each term in the expansion expresses the constitution of the individual. In the case of the dihybrids, the second term is $2u(2v + w)$. Of the six individuals represented by this term, four are generated by combinations of u and v; two by u and w. For a more complete analysis of Pearson's expansion, the reader is referred to the original. It is suggested that special attention be paid to the continuation of the above quotation and to the corollaries i and ii. This exceptionally important paper is well worth reading in its entirety. Lack of space alone prevents us from giving it the prominence in this article it so justly deserves.

We now proceed to Table 2 which is of considerable practical value and which results from rotating Table 1 so that the diagonals become columns. This tabulation makes apparent the relation of dominant characters and the numbers in parentheses.

Every instance of the number 1 represents a unit class containing individuals displaying only recessive characters; every instance of the number 3 represents classes of trios displaying exactly one dominant character; number 9 two dominants; etc. In short, letting d be the number of dominant characters (also the degree of hybridism):

$$3^d$$

represents the number of individuals in each class displaying d dominant characters. Note that $3^0 = 1$.

TABLE 2

Phenotypes: increasing dominants

							$(3)^1$	$(1)^1$	4
						$(9)^1$	$(3)^2$	$(1)^1$	16
					$(27)^1$	$(9)^3$	$(3)^3$	$(1)^1$	64
				$(81)^1$	$(27)^4$	$(9)^6$	$(3)^4$	$(1)^1$	256
			$(243)^1$	$(81)^5$	$(27)^{10}$	$(9)^{10}$	$(3)^5$	$(1)^1$	1024
		$(729)^1$	$(243)^6$	$(81)^{15}$	$(27)^{20}$	$(9)^{15}$	$(3)^6$	$(1)^1$	4096
	$(2187)^1$	$(729)^7$	$(243)^{21}$	$(81)^{35}$	$(27)^{35}$	$(9)^{21}$	$(3)^7$	$(1)^1$	16384
$(6561)^1$	$(2187)^8$	$(729)^{28}$	$(243)^{56}$	$(81)^{70}$	$(27)^{56}$	$(9)^{28}$	$(3)^8$	$(1)^1$	65536
.	.	.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.	.	.

GENOTYPES

A group of individuals possessing the same genes belong to the same genotype. Thus there are three distinct genotypes in the case of the monohybrids: AA, Aa, aa, since, Aa = aA. Likewise there are 9 distinct genotypes in the case of the dihybrids, for there are three combinations of AA with BB, Bb, bb; three combinations of Aa with BB, Bb, bb; and lastly three combinations of aa with BB, Bb, bb. In short, the number of distinct genotypes is

$$3^d$$

where d is the degree of hybridism. Thus the number of genotypes is the same as the number of individuals in each class displaying d dominant characters. This is analogous to a preceding result which was that the number of phenotypes, 2^d, is identical with the number of dominant characters being considered in a given case.

As an illustration a table of genotypes (Table 3) for trihybrids is given. This is the familiar square graph representation. The numbers within the graph represent distinct genotypes. Note the perfect symmetry along the main diagonal.

Table 4 is based on Table 2, and contains the classes, ratios, and membership of both phenotypes and genotypes. Thus the table tells at a glance how many distinct genotypes are to be found in each phenotype for every degree of hybridism. The symbolism employed is identical with that of Tables 1 and 2, and hence needs no further explanation.

To illustrate. In the case of dihybrids, there is one phenotype theoretically containing 9 members. These are subdivided into one genotype of 1 member, two genotypes of two members each, and one genotype of 4 members. We shall now prove that this table can be extended to any desired degree of hybridism. Having already discussed the phenotypes, the present discussion is limited to genotypes. First we consider column 2 from the right.

Since the individuals in this column display only one dominant character, if G is any group of characters, the composition of G will have either one dominant gene or two dominant identical genes. In the latter case, the two identical genes will generate as many unit types as there are characters involved; i.e. degree of hybridism. In the former case, the dominant gene and its corresponding recessive will generate as many types of two members as there are characters involved. Therefore the genotypic items in column 2 will always be

$$(1)^d(2)^d$$

TABLE 3

A table of genotypes for trihybrids

	ABC	ABc	AbC	Abc	aBC	aBc	abC	abc
ABC	1	2	3	4	5	6	7	8
ABc	2	9	4	10	6	11	8	12
AbC	3	4	13	14	7	8	15	16
Abc	4	10	14	17	8	12	16	18
aBC	5	6	7	8	19	20	21	22
aBc	6	11	8	12	20	23	22	24
abC	7	8	15	16	21	22	25	26
abc	8	12	16	18	22	24	26	27

For instance, for monohybrids we have AA $(1)^1$ and Aa, aA $(2)^1$. Dihybrids: AbAb, aBaB $(1)^2$; Abab, abAb and aBab, abaB $(2)^2$. Trihybrids: aabbCc, aaBbcc, Aabbcc can be arranged into 3 types of two each $(2)^3$; AAbbcc, aaBBcc, aabbCC can be arranged into 3 types of 1 each $(1)^3$.

Analogous arguments verify the correctness of the other columns. Note that the ratio of the coefficients in each column is always the same. It is still necessary to prove that the series of numbers representing the individuals within each genotype proceeds as shown in the table; i.e. 1, 2, 4, 8, 16, . . .

By adding a new character, each class

TABLE 4

Phenotypes—Genotypes

PHENOTYPE ...... $(3)^1$ ...... $(1)^1$
GENOTYPE $(1)^1$ $(2)^1$ $(1)^1$

PHENOTYPE .... $(9)^1$ ...... $(3)^2$ ...... $(1)^1$
GENOTYPE $(1)^1$ $(2)^2$ $(4)^1$ $(1)^2$ $(2)^2$ $(1)^1$

PHENOTYPE .... $(27)^1$ .... $(9)^3$ ...... $(3)^3$ ...... $(1)^1$
GENOTYPE $(1)^1$ $(2)^3$ $(4)^3$ $(8)^1$ $(1)^3$ $(2)^6$ $(4)^3$ $(1)^3$ $(2)^3$ $(1)^1$

PHENOTYPE .... $(81)^1$ .... $(27)^4$ .... $(9)^6$ ...... $(3)^4$ ...... $(1)^1$
GENOTYPE $(1)^1$ $(2)^4$ $(4)^6$ $(8)^4$ $(16)^1$ $(1)^4$ $(2)^{12}$ $(4)^{12}$ $(8)^4$ $(1)^6$ $(2)^{12}$ $(4)^6$ $(1)^4$ $(2)^4$ $(1)^1$

PHENOTYPE .... $(243)^1$ .... $(81)^5$ .... $(27)^{10}$ .... $(9)^{10}$ ...... $(3)^5$ ...... $(1)^1$
GENOTYPE $(1)^1$ $(2)^5$ $(4)^{10}$ $(16)^5$ $(32)^1$ $(1)^5$ $(2)^{20}$ $(4)^{30}$ $(8)^{20}$ $(16)^5$ $(1)^{10}$ $(2)^{30}$ $(4)^{30}$ $(8)^{10}$ $(1)^{10}$ $(2)^{20}$ $(4)^{10}$ $(1)^5$ $(2)^5$ $(1)^1$

on a given level generates *two* new classes on the next lower level; one through CC, the other through Cc (= cC). One of the new classes will be characterized by members having only dominant characters, and moreover it is evident that this is a unit class. The unit class, U, of the preceding level generates a new class of two members: UCc, UcC. Since every other class of a given degree has more than one member, the number of members of the new classes generated by the multiple-membered classes remain the same when combined with CC and are doubled when combined with Cc and cC. In brief, if there are m members in a given class, then by increasing the degree of hybridism by one character, there will be m members in the new classes characterized by CC, and $2m$ members in the new classes characterized by Cc. Hence with the addition of each character, the series representing the membership in the types is increased by one term, and the series itself is a geometrical progression with 2 as the common ratio. The difficulty of expressing this change is due to the fact that, upon addition of another character, complexities arise because of the uniqueness of CC.

Example. The first item from the left in Table 4 represents the monohybrids having at least one dominant character. There is one genotype of one member: AA, and one genotype of two members: Aa, aA. By adding a character B, new types are generated which are represented in the first item from the left, dihybrid row.

(1) AABB	(2) AaBB	(3) aABB
(4) AABb	(5) AaBb	(6) aABb
(7) AAbB	(8) AabB	(9) aAbB

Number 1 is the member of the unit type. Numbers 2 and 3 are members of a new type determined by A, a, B, B. Numbers 4 and 7 are members of a new type determined by A, A, B, b. The remaining four individuals constitute the fourth type. Therefore the series expressing the membership in these types is 1, 2, 4.

We are therefore permitted to extend the table to any degree of hybridism. If d is the degree of hybridism, there will be $d + 1$ terms in the membership progression of the first item from the left in any row. The progression is geometrical, with 2 as the common ratio.

It should be noticed that the coefficients of the genotypes in the first column from the left for each degree of hybridism are the same as the coefficients of the phenotypes for the entire degree. The reasoning behind this is the same as that behind the determination of the phenotypic coefficients.

The value of Table 4 lies in this, that it can be read for any degree of hybridism by simple extensions of the various series, which is obviously simpler than treating each degree separately.

With regard to the determination of genotypes, Babcock and Clausen (2, p. 98) state

> It may be noted that there is one phenotype in each class homozygous for all its factors. In this class starting with this phenotype, we double the number of individuals each time an additional pair of factors becomes heterozygous. . . . Thus (in the case of the trihybrids) there are three genotypes possible with only one heterozygous factor, and there will be two individuals of each of these, there will be three different genotypes having two heterozygous factors, and each of these will be represented by four individuals, and finally there is only one genotype with three heterozygous factors and it is represented by eight individuals.

It seems clear that Bateson and Babcock and Clausen logically and chronologically owe their approach to the topic of this paper to Pearson, although Bateson credits an anonymous mathematical friend for providing him with the binomial expansion. The mathematical relationships and conclusions of this elementary part of Mendelian theory could be derived from

Pearson alone. [de Vries (15) also considered the multinomial 1A + 2H + 1L, where A = active, L = latent, H = hybrid.] That still another discussion of it is here being presented is due to our belief that we have arranged the results in the simplest form yet published and because our tables offer completely the material they were designed to offer without mathematical exertion on the part of the reader. Nevertheless, we would like to point out that the discussions of the tables present, in some respects at least, a fresh point of view, and that our reference to Symbolic Logic as an instrument for the analysis of the theoretical relationships obtaining among genes is highly suggestive of the possibility of employing a new and powerful technique to the mathematical study of genetics. Woodger, in this QUARTERLY in 1930, pointed out a way of applying the modern logical tools to the analysis of biological concepts, while Serebrovskii (12) has enumerated and defined several operations for the algebraic development of a calculus of genes. There can be no doubt that a rich field awaits the logician who turns to an investigation of the mathematics of genetics.

[The authors wish to acknowledge their indebtedness to Prof. Raymond Pearl for his suggestions and criticism in connection with the preparation of this paper. Thanks are also due to our colleague Prof. John Paul Givler for urging that this paper be written and submitted for publication.]

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ESSAYS ON EVOLUTION

II. ON THE EFFECTS OF SELECTION ON SOCIAL INSECTS

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THERE are two conflicting tendencies in recent developments of the mathematical theory of the behavior of wild populations. On the one hand there is the view that selection is ever-present and is the only effective agent in altering the constitution of populations. On this view selection operates only on individuals, and the members of a group are not thought of as occurring in semi-independent subgroups (Fisher 1930). On the other hand there is the point of view expressed by Wright (1932), according to which the most favorable conditions for evolution are those in which the members of a large population do not interbreed at random, but exist in more or less independent subgroups, which undergo exchanges of genes at times but are also effectively in competition with each other. On this latter view there is an opportunity, lacking on the first view, for selection to bring about the establishment of characteristics favorable to the group but unfavorable to the individual.

The social insects, such as termites, ants, and many bees and wasps, furnish examples in which such characters unfavorable to the individual have in fact become established. In terms of natural selection, a "favorable" character is of course to be taken as meaning a character that leads to the production of more descendants. The sterile castes of the insects named have, therefore, developed a character that is unfavorable by definition. It is sometimes argued that in this special case the rule still holds, since here the colony, rather than the individual insect, is the unit in terms of natural selection, and it may be supposed that the colony produces more offspring as a result of the division of labor associated with the sterile castes. It is clear, however, that all the social insects have arisen from solitary forms in which the sterile caste was absent (Wheeler, 1928). It follows that evolution must have resulted in an increase of sterile individuals. At some point in the history of the race there must have been a change from the individual to the colony basis of selection. Unless this change be supposed to have been a sharp one and to have been associated from the first with the necessary genetic adjustments, there must have been an intermediate stage in which some element other than the strict operation of reproductive selection was effective.

In fact, such intermediate stages still exist, and are familiar to all students of the social insects. In order that the colony be the unit in terms of natural selection, and that the existence of the sterile caste offer no difficulties to the advocates of pure selection, it is necessary that each colony have a single fertile queen and that the sterile individuals all be closely related to the queen (presumably her offspring). This is the standard account for the honey-bee, though even here it is

clear that workers may at times produce offspring; in other social forms wide deviations from this condition are frequent.

In what follows I shall confine myself to the ants, though evidently somewhat similar relations occur in certain of the termites at least. For detailed summaries of the life-histories and for bibliographies the reader is referred to Wheeler (1910, 1928) and to Donisthorpe (1915). Fertile workers do not appear to be rare among the ants; usually they produce only male offspring, but there are some satisfactory records of female offspring—the latter presumably due to failure of chromosome reduction in parthenogenetic eggs, since it seems clear that such fertile workers do not mate. The production of males is, however, enough to put these workers into reproductive competition with the queens; and therefore to upset the view that the colony is the unit of selection, to the exclusion of individual selection within the colony. Even more to the point is that, in many ants, more than a single fertile queen is present in a given colony. In such dominant genera as *Myrmica*, *Crematogaster*, and *Tapinoma*, and in the *rufa* and *sanguinea* sections of the genus *Formica*, flourishing nests regularly contain many fertile queens. In some (probably in all) these cases the colony is initiated by a single queen, but newly fertilized queens are adopted by established colonies after the mating flight occurs. In the case of *Formica rufa* at least, there is evidence that these adopted queens need not have been produced in the nest that adopts them—they may even belong to a distinct variety. Here and in *F. exsectoides* it seems probable that the colony long outlives the queen that originally established it, its fertility being dependent upon a whole series of queens that have no necessary genetic relationship to each other or to the founder.

Under these conditions there is evidently an opportunity for individual selection. A queen that produces unusually efficient worker offspring will have to share the benefits of their activities—i.e. her fertile offspring will have no advantage over those of the other queens living in the same nest. A queen that produces a relatively high proportion of fertile offspring will leave more descendants—at the expense of the nest economy, and in the long run to the detriment of the species. This possibility seems to constitute a serious danger to such ants, and may therefore be examined more closely.

There are ants in which the worker caste has disappeared entirely (*Anergates*, *Wheeleriella*, etc.), so that mutations of this sort are evidently possible. If such a one occurs, then a queen of the new type that is adopted by a normal colony will thrive at the expense of the worker offspring of her foster-sisters, but will produce more than her share of the sexual forms arising in the colony. Such a gene may be expected to spread through the population rapidly at first, but must soon be checked by the occurrence of too high a proportion of sexual forms to be supported by the reduced number of workers that will be available. It seems clear that the net result must be a decrease in the efficiency of the colonies and therefore of the total population of the species in the affected area. This will, evidently, leave such a region likely to be invaded by members of the species coming from places where such a process has not happened to occur.

Selection must, then, be thought of as operating, in these forms, on at least three different levels: on the individuals, on the colonies, and on the populations with-

in an area. The interrelations between these three are obviously complex. Similar distinctions apply in the case of another social animal—namely man. Here, of course, the situation is even more complex, and I do not have the intimate knowledge of the data necessary for a profitable discussion.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that The Quarterly Review of Biology *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* The Quarterly Review of Biology, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

TIMING LIFE

Being a review of *Biological Time* by P. Lecomte du Noüy. Foreword by Alexis Carrel. New York (Macmillan Company) 1937. $7\frac{3}{4} \times 5\frac{1}{8}$; xiv + 180. $2.00.
By Kurt Edward Rosinger, Woman's College, University of North Carolina.

Here indeed is an important book. Heralded two years ago by Carrel in his *Man, The Unknown*, it appeared late in the spring of 1937, rich with suggestion for intensive research by the mathematically inclined biologist or the biophysicist, fraught with interest for the philosopher who grounds his investigations in science, and tantalizing with speculative possibilities for the psychologist and sociologist. Nor is its central thesis, the determination of units for a biological time, of purely theoretical value. To the satisfactory solution of the problem herein presented accrue practical consequences of indubitable importance to the practicing physician. Because it proposes a many-faceted problem, even though it does not solve this problem, the book is important.

Although the author has divided his book into three sections, its subject-matter is logically divisible into two: (1) the statement and partial development of a purely technical and experimental problem in mathematical biology (Chapters IV-VII) and (2) a philosophical consideration of that problem and of its solution. This philosophical part is itself divisible into a preliminary presentation of the difficulties and limitations attending the application of a rigorous quantitative physico-chemical methodology to the domain of biology (Chapters I-III), and a philosophical deduction from the solution of the biological problem (Chapters VIII and IX). The former belongs primarily in the field of the logic of science, and would be of considerable value to the logician concerned with the methods of science. The latter is essentially metaphysical, being a brief analysis of the concept of time and of the differences between physical or sidereal time and biological time, a concept much discussed by Carrel and developed by Lecomte du Noüy. Like all metaphysical discussions, it bares itself readily to objections, and bases itself upon assumptions open to the critical attack of philosophers apparently far better trained in metaphysical technique than is the author. Yet it is doubtful that even these would cast sharp animadversions upon these two chapters for, although Lecomte du Noüy is at times naïve he presents in his concept of biological time an idea too rich in metaphysical possibilities to be subordinated to the pleasure of disparaging its promulgator. Nevertheless, this is probably the weakest part of the book.

The biological problem involves an attempt to discover an index of the rate of cicatrization of wounds and an index of the rate of proliferation of cells *in vitro*, a comparison of these indices, and as a consequence a correlation of these indices with a patient's calendar age to determine,

if possible, his age in terms of physiological units of time. The problem was first suggested to the author by Carrel when both were seeing military service in 1914. It was limited at first to the developing of a method for measuring the surface areas of wounds. By performing repeated measurements as the wound was in the process of healing, the rate of healing might be determined. If, moreover, it should be shown that this rate is a function of determinable variables, a general formula for cicatrization could be set up. Carrel had begun this study in a more or less non-quantitative manner, but precise mathematical methods were obviously indicated. These were to be supplied by Lecomte du Noüy.

At this point, one practical value of this research may be remarked upon. Satisfactory results would throw considerable light upon the nature and mechanism of the cicatrizing process, upon the relative merits of various dressings with respect to their inhibiting or stimulating influence upon healing, and might bring into relief the existence of other retarding or accelerating factors. The testing of antiseptics was, as a matter of fact, performed at an early stage in this research, with the result that Dakin's Solution Number 30 and Dakin's Chloramine-T were chosen from among almost two hundred substances at hand at the military hospital, as the most desirable.

It is beyond the scope of this review to give an account of the ingenious cerebrations through which the author arrived at his formula, nor the means employed in measuring the wounds and in constructing curves and tabulations for expressing the process of healing. Suffice it to say that after giving a clinical picture of the ordinary surface wound—knowledge possessed by the physician, but necessary information to readers without medical training—the author proceeds to present the steps through which he arrived at his formula. The excellence of the result is attested to by the fact that the difference in the time of the calculated and the actual period of cicatrization in a supposedly characteristic case was only two days over a period of ninety days; certainly an insignificant error under the circumstances. As an example of the quantification of biological methods, this account in *Biological Time* is well worth the efforts of any scientist or philosopher interested in this problem.

From here the author progresses to a further analysis of his formula and to what can be derived from it. An index of cicatrization, relatively constant for a given wound, had been discovered. The result now to be desired was to find a more general formula; one not limited to a specific wound. This the author proceeds to do, based on his observations that the index is a function of the area of the wound and the age of the patient. Small wounds cicatrize more rapidly than do larger ones, and those of young patients more rapidly than of older men. There appear to be no other relevant factors except inveterate alcoholism, diabetes, syphilis, and similar ones. From many experiments and from the study of curves describing the rate of healing, Lecomte du Noüy derived an empirical mathematical formula satisfying his needs. Hence the author claims that it is possible to determine the (physiological) age of a patient when the size of the wound is given and when it is known that other disturbing factors are absent.

The discussion of tissue culture *in vitro* is admirably clear and interestingly written, but is merely a brief review of material already to be found in the literature. Its value, in contrast to classical cytology, is well defended by showing that with this methodology one can observe cells active in their environment, and not dead upon a slide. Thus it is possible to study their reactions to modifications in the surrounding medium as a function of time. Moreover, knowledge of the physiology and pathology of cell life is greatly increased. But the result which is of particular concern to the topic of this book is that the growth index deduced from Carrel's experiments of cells *in vitro* is closely related to Lecomte du Noüy's own index of cicatrization. From these a new and distinct time, expressed in physiological units, can be derived.

As has been remarked upon above, the problem of determining time through the physiological activities of organisms has not been settled; it has rather been proposed and delineated. Much is yet to be done.

The experimental data must be greatly augmented. The possible relevancy of factors other than age and size must be investigated. The influence of the shape of wounds to the activity of cicatrization must be further studied. It may be well to know what effect upon the healing process the size of the wound has relative to the total surface area of the body. The mathematical application itself needs critical analysis. Many other facets of this problem exist which could occupy the attention of scientists, not the least of which is the search for other physiological processes whose measurement might aid in the determination of biological age, or might compel a revision of the concept.

Because of the scientific nature of this QUARTERLY, the philosophical sections of this book will not be reviewed in detail. The preliminary metaphysics expounded is neither original nor unusual. It is, however, plausible reasoning, based on a knowledge of physics with a Bergsonian inclination. It is well written but elementary. Of chief interest is the comparison of physiological to sidereal time. As is to be expected, the two are not congruent. Since at different calendar ages it takes different lengths of sidereal time to accomplish the same amount of cicatrization, it follows that in terms of physiological units, sidereal time apparently ceases to flow at a constant rate. Here relativity enters the domain of biology. However, the author prefers not to consider time as continuous, but rather as discrete. Time, like matter, electricity, and energy is granular, and "its continuity is only the statistical appearance given it by individuals."

A word should be said about the method of presentation employed in this book. Instead of being a statement of results and of interpretations of these, the author leads the reader along the steps of his cogitations, making of his research an attractive scientific adventure, although one in which only the technically trained reader can join.

BRIEF NOTICES

EVOLUTION

A STORY OUTLINE OF EVOLUTION.
By Chas. W. Grimes. C. P. Hoagland, Somerville, New Jersey. $2.00. $7\frac{3}{4}$ x $5\frac{1}{4}$; 286; 1937.

This work is well named. Its extremely simplified language entitles it to be called a story and its lack of discussion of specific theories makes it a true outline.

The field of general evolution is an extremely wide one, for the doctrine first enunciated by the biologists has since been extended to include all the sciences and all the arts as well. An author who attempts to cover all of these must necessarily spread himself pretty thin. Consequently we find here no discussion of the factors of evolution, but only of the facts. The author has not always checked up on his statements, however, for he repeats the popular superstitions that the tadpole's tail is detached during metamorphosis, that snakes charm their prey with a hypnotic glance, and that witches were burned in America. Another feature not to be commended is the use of the word "vertebrate" to mean, not an animal with a backbone, but the backbone itself.

The author has been more successful in his treatment of cultural than in that of organic evolution. The chapter dealing with the evolution of instrumental music is perhaps the best in the book, but those dealing with the alphabet and the making of artifacts are nearly as good. The unification of the various fields is well accomplished. The blurb on the cover recommends this book as a gift for inquisitive youngsters, and with this opinion the present reviewer concurs.

DARWIN'S THEORY APPLIED TO MANKIND.
By Alfred Machin. With a Foreword by Sir Arthur Keith. Longmans, Green and Co., New York. $3.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; xxiv + 284; 1937.

In this sequel to *The Ascent of Man by Means of Natural Selection*, Mr. Machin finds expression for his twenty years' study and observation on the application of Darwin's theory of natural selection to mankind. Like the earlier volume, this

work is based on the author's firm conviction that if we believe that natural selection accounts for the origin and evolution of plants and animals, then we must believe that it accounts also for the evolution of man. Mr. Machin does not rest with a meagre explanation of his theory, but marshals forth fact after fact of undisputed physiological, psychological, biological and paleontological phenomena to prove his reasoning.

The author maintains that the instincts represent the original state of mankind, and that the other elements of our moral code have been developed in the process of evolution. As these elements have proved themselves either a benefit or a hindrance to the well-being of mankind, they have been retained or lost. Hence, modern man represents a bundle of survival values acquired from the different stages through which he has passed and is passing.

The book is well written in a clear and logical style, and will undoubtedly be stimulating to the mind of every deep-thinking biologist.

STRUCTURAL MODIFICATIONS IN THE HAWAIIAN GOOSE (NESOCHEN SANDVICENSIS). *A Study in Adaptive Evolution. University of California Publications in Zoology, Volume 42, Number 1.*

By Alden H. Miller. University of California Press, Berkeley. $1.00. 10¼ x 6¾; 79; 1937 (paper).

This study of the musculature and skeletal anatomy of the Hawaiian goose, including frequent comparisons to related North American genera, has as its object not only the description of anatomical features but more especially functional interpretation. The author undertakes a physiological anatomical analysis of limb mechanics, particularly hind limbs, in order to correlate certain characteristic differences in bulk of muscle with the habits and natural history of the species. The Hawaiian goose does less flying and swimming than any other species, and lives mostly on dry, barren, uneven, lava uplands. Therefore its limb structures represent considerable modification from the North American representatives of the genera. There are 115 characteristics differentiating this goose (*Nesochen sandvicensis*) from other species.

ORGANIC EVOLUTION PROVABLY FALSE.

By H. R. Kindersley. Thynne and Co., London. 1s. 7¼ x 4⅞; 49; 1937.

A vituperative attack upon the theory of organic evolution and its supporters, based on the argument of the immutability of breeding species; namely, that members of distinct species cannot, as a rule, interbreed and thus create new species. Experiments prove, the author claims, that species do not change, nor is there any evidence that new species emerge from established ones in natural ways. From this it follows that the theory of organic evolution collapses. Feeble stuff bred out of Ignorance by Emotion.

PALEONTOLOGY OF THE PLEISTOCENE OF POINT LOMA, SAN DIEGO COUNTY, CALIFORNIA. *Transactions of the San Diego Society of Natural History, Volume 8, No. 24.*

By Robert W. Webb. Society of Natural History, San Diego, Calif. 10¼ x 7; 12; 1937 (paper).

GENETICS

THE FIGHT FOR OUR NATIONAL INTELLIGENCE.

By Raymond B. Cattell. Introductions by Lord Horder, Major Darwin and F. P. Armitage. P. S. King and Son, London. 8s. 6d. net. 8¾ x 5½; xx + 166; 1937.

From the first to the last page of this book the author relentlessly pursues but one objective. This is to arouse the English people to the realization that the intelligence of the population is declining and that drastic preventive measures are necessary. The author presents three sets of facts. (1) The distribution of the I.Q.'s obtained on almost 3000 urban and almost 900 rural school children of 10 years of age. (2) A graphic representation of the distribution of the size of the sibship in the families of these children. This

graph indicates that on the average children with low I.Q.'s belong to large families and children with high I.Q.'s belong to small families. (3) A positive correlation of .73 between the I.Q.'s of parents and of children in 100 families. From these facts, mainly, the author reasons: intelligence is primarily inherited, people with low intelligence reproduce more children than those with high intelligence, *ergo*, in successive generations the percentage of people with low intelligence will steadily increase while that of persons with high intelligence will decrease.

Having reached this conclusion he proceeds to paint in very dark colors the future of England, its steady deterioration and subsequent destruction. Only eugenic measures can save the country from its fate, he says, and reviews in detail a number of plans all directed towards stimulating the reproductivity of persons with high I.Q.'s.

The author foresees that his conclusions may be criticized. So, in a manner verging upon political demagoguery, he already inveighs against future critics. At the risk of being called a "rhetorical advocate of *laissez-faire*", one cannot overlook the fact that the burden of proof still lies with him, and that the given data, in view of what is known regarding the inheritance of complex characters, do not wholly prove his conclusions.

The Etiology of Mental Deficiency with Special Reference to Its Occurrence in Twins: *A Chapter in the Genetic History of Human Intelligence. Psychological Monographs, Volume 48, No. 4.*
By Aaron J. Rosanoff, Leva M. Handy and Isabel R. Plesset. Psychological Review Co., Princeton, N. J. $2.00. 10 x 6¾; 137; 1937 (paper).

This study is based upon the given case reports of 366 pairs of twins (126 monozygotic, 102 dizygotic and 138 pairs of opposite-sex twins), with mental deficiency in one or both twins of each pair. The data have been classified in various tables according to type of twin, sex, uncomplicated mental deficiency, mental deficiency accompanied by other mental disorders, and whether one twin is affected or both are affected similarly or dissimilarly. One section of the monograph is devoted to a discussion of sex factors in intelligence and in the etiology of mental deficiency, and in another the material has been further analyzed and discussed according to a suggested classification of the etiological factors of mental deficiency based on the periods in which these factors are operative: pre-germinal (hereditary), germinal, embryonic, fetal, intranatal, and postnatal factors. In the evaluation of their accumulated data and in their espoused theory of mental deficiency based upon it, the authors have remained cognizant of certain shortcomings in quality and amount of the raw material, but feel nevertheless that their findings hold promise as a basis for further intensive study of the problem.

Twins: *A Study of Heredity and Environment.*
By Horatio H. Newman, Frank N. Freeman, and Karl J. Holzinger. University of Chicago Press, Chicago. $4.00. 9¼ x 6¾; xvi + 369; 1937.

In this study a biologist, a psychologist and a statistician pool their intellectual resources in an attempt to produce a solution of sorts to the nature-nurture problem. They have not tried to derive a genetic-environmental ratio for the develment of the individual; their purpose has been merely "to secure evidence on the extent to which the characteristics of human beings, especially their ability and behavior, are determined by their genetic constitution and the extent to which these characteristics are influenced by the conditions of the environment."

Ten years have elapsed since the authors agreed to collaborate. During that time they have collected and analyzed three types of cases in far greater numbers than have ever before been studied. These types include 50 cases of identical twins reared together, 50 cases of fraternal twins reared together, and—the *pièce de resistance*—19 cases of identical twins separated in infancy and reared apart.

The problem is extremely complicated

and involves many factors. We have not space here to consider the inevitably complex (though not startling) conclusions at which the authors ultimately arrive. Suffice it to say that those interested in the problem will find much valuable material in this volume. The text is abundantly illustrated with tables and photographs and is adequately indexed.

ERBKRANKHEIT UND FERTILITÄT. *Micropathologie der Spermien erbkranker Männer.*
By H. Stiasny and K. D. J. Generales, Jr. With an Introduction by Erwin Gohrbandt. Enke Verlag, Stuttgart. RM. 27 (paper); RM. 29 (bound). $9\frac{1}{2} \times 6\frac{1}{4}$; xii + 163; 1937.

This is an important introduction to the study of the fertility of men afflicted with hereditary diseases, such as congenital imbecility, various types of insanity, epilepsy, blindness, deafness, and others listed under the German sterilization edict of July 14, 1933. It is devoted largely to the authors' own work, in the hospital "Am Urban" in Berlin, on analyses of spermatozoa of the types of men recommended for sterilization. In some hereditary diseases the number of normal spermatozoa found was only 37.15 per cent as compared with 81 per cent, on the average, among normal men. The greatest number of abnormal spermatozoa were found among the congenital idiots and the alcoholics. The idiots likewise had the lowest fertility rates (1 child apiece, on the average). The alcoholic groups, due to raised *libido*, had done better toward contributing to the census totals. Some information on the history and technique of sterilization, on normal spermatogenesis, and analyses after sterilization is included. The book is provided with illustrations, including 16 polychromatic tables. It contains a bibliography but is not indexed. It can well serve as a basis for further profitable research.

ANIMAL BREEDING PLANS.
By Jay L. Lush. Collegiate Press, Inc., Ames, Iowa. $3.00. $9\frac{1}{4} \times 6\frac{1}{4}$; viii + 350; 1937.

The breeder of animals for economic purposes is still engulfed in a maze of practical difficulties for which science has as yet to offer solutions. But since the business of animal breeding must go on while further scientific knowledge is being acquired, it is well to bring together for study and comparison such methods as are at present available for improving the heredity of farm animals. Such has been the purpose of this author in writing a text for college students who have already had courses in genetics, embryology, anatomy and physiology of farm animals, herdbook study, history of breeds and stock judging. Genetic principles involved in animal breeding are reviewed, and breeding plans based on selection, relationship and somatic likeness are discussed pro and con. Other topics concerning breeding plans and relating to reproduction are considered briefly. Each chapter is summarized and followed by a list of references. The book includes subject and author indices.

CONTRIBUTIONS TO HUMAN HEREDITY. *In Honour of Maria Anna Van Herwerden.* Containing the following articles: *Die Bedeutung von Dr. Maria Anna van Herwerden für die Genetik und die Eugenik*, by Tine Tammes; *L'Examen Médical Prénuptial, ses Modalités et ses Consequences*, by G. Schrieber; *Ueber familiär-erbliche Fälle von seniler Makula-degeneration*, by P. J. Waardenburg; *Recent Progress in Blood-group Investigations*, by R. Ruggles Gates; *Erbliche Belastung in der Durchschnitts-Bevölkerung*, by G. P. Frets; *Eine Familie mit Kraushaar*, by J. Sanders; *Abortive Differentiation of the Ear Vesicles*, by Kristine Bonnevie; *The Coil-spring Properties of Chromosomes*, by Harry H. Laughlin; *Ueber eine verbesserte Methode der Berechnung der Abhängkeit von Merkmalen (Vierfelderrechnung)*, by H. Reichel.
Martinus Nijhoff, The Hague. Gld. 4. $10 \times 6\frac{1}{2}$; 156; 1936 (paper).

The Editors of *Genetica* have honored the late Maria Anna van Herwerden by devoting two numbers of their journal to articles on genetics and human heredity. The eight papers appearing here represent

excellent pieces of scientific investigation, and mostly make definite contributions. Each paper is well illustrated and summarized, and the majority contain extensive bibliographies. There is a table of contents, but no index.

GENERAL BIOLOGY

Mysteries of Natural History.

By E. L. Grant Watson. Frederick A. Stokes Co., New York. \$1.75. 8 x $5\frac{1}{4}$; x + 244; 1937.

The readers of The Quarterly Review of Biology do not need to be told that a vast number of biological problems have yet to be solved. Furthermore the normal reaction of these readers to this obvious fact is always to keep on working a little harder and more intelligently to answer the difficult questions, one by one. It is by persistently maintaining this attitude in the face of apparently insuperable obstacles and difficulties that science has made the progress it has in understanding and controlling natural phenomena. So long as priestly mysticism was the dominant attitude of man towards nature no such progress was made.

The patient preference of scientifically minded men for the precise clarity of real knowledge is not yet universal among mankind. To many soft-minded folk it still seems much pleasanter to look upon the facts of life as transcendental mysteries, and bemuse themselves in beautifully phrased speculations about the wonderful goodness and loveliness of it all. Some literary folk—and especially those a bit tainted with religious mysticism—have always been addicted to this type of intellectual masturbation. Drugged by their own verbiage into a gentle narcosis, they like to think that their lucubrations are setting bounds to science and to what it can know or teach.

The book that has led to these remarks is beautifully written, and has enjoyed a considerable popularity in England, both in magazine and book publication. The fact, however, is that its "absorbing style" is made the vehicle of an extremely subtle, and therefore the more vicious, undermining of biology and science in general. Space is lacking for more than one illustration of the technique, though various others and even more deplorable ones might be taken. The main point of a chapter entitled "Wise Worms," which deals with the behavior of the larvae of cerambycid beetles (which are *not* worms), is the fact that the pupae are generally so oriented that the head of each emerging imago will be pointed outwards, rather than towards the center of the tree. A very loosely constructed straw man is set up and then promptly knocked down again, to prove that natural selection as an explanation of this beautiful bit of teleology is, like most science, slightly comic. Then we are told that "the only alternative solution would be to postulate a life inspiration, a life wisdom, within the grub." Maybe so. But it will occur to biologists that some day a behaviorist and physiologist is surely going to do a competent and penetrating job of work on this problem, and may quite probably then find that the adaptation is *not* 100 per cent statistically perfect, and that its explanation when it does come off lies in a predictable and controllable tropistic reflex type of behavior in which neither larval "inspiration" nor "wisdom" play any discernible part.

Enough has been said to make it plain that The Quarterly Review of Biology offers neither aid nor comfort to mystic purveyors of natural theology, however tastefully they may besauce their garbage or flavor it with the seductive elegance of their diction.

The Nature of Growth. *A Logistic Inquiry.*

By Frederick S. Hammett. The Science Press, Lancaster, Pa. 75 cents. $9\frac{1}{4}$ x 6; 61; 1937 (paper).

This brochure is a series of numbered paragraphs, varying in length from one line to twenty-four, and in intelligibility, from not so much to none at all. We challenge those of our readers who have a taste for philosophical discussion to attempt the following:

217 If it may be assumed that in the beginning living matter was a formless loose collection of amino acids

in discontinuous conjunction with but a single activity of union and disunion—and if it may be assumed that a concatenation of conditions and happenings brought to such a gathering a group of other compounds wherewith combination occurred such as to produce a new property—that of pulsation or alternate contraction and expansion—possibly because of the endowment of the mass with colloidal properties capable of shifting in state with shift in environmental conditions—then along with this because of the properties of chemical elements in combination and molecules in aggregates there might have been such an oriented coacervation or organization of the condensate as would have produced the first and primitive muscle tissue—not because of the form but because the function of pulsation comprised chemical molecules in combination which took the form thereof

We have found, after careful research, that the author's excogitations first give clear indications of emerging from their obscure prose wrappings when the paragraph encasing a specimen is approximately nine lines. From that point on, scaling downwards, the increase in luminosity is very rapid and approaches an asymptote, as the following illustrates:

"33 No single formula yet devised is adequate either to correlate the potencies already expressed or to predict those which the future may produce"

Chemical processes are referred to, biological terms are freely employed, and all without benefit (among other things) of much punctuation.

[*Editorial note:* Reginald the Office Boy, whose voice is now in process of changing, with the usual concomitants of greater seriousness of intellectual outlook generally and particular concern as to whether his collar is too dirty to wear one more day, likes this book. He says the only trouble is that biologists like the reviewer are just too dumb to understand a great book. Maybe Reginald is right. We venture no opinion, but the case does bring to mind Seneca's sage remark:

Quidquid excessit modum,
Pendet instabili loco.

Doctor Hammett's book does indubitably go beyond the present biological mode.]

ANIMAL COMMUNITIES IN TEMPERATE AMERICA *as Illustrated in the Chicago Region. A Study in Animal Ecology. Second Edition.*
By Victor E. Shelford. University of Chicago Press, Chicago. $3.00. 9½ x 6¼; xiii + 368 + 1 folding map; 1937.

In 1913 the early development of animal ecology was greatly stimulated by the original publication of this book. At that time there was a crying need for a rounded study of animal habitats. This was especially true since the plant ecologists were already well established and were rapidly outpacing their zoological colleagues. Shelford's book served the real purpose of collating and synthesizing a great deal of field knowledge. The approach was novel. Instead of dealing textbook fashion with communities drawn from all over the world, the author delineated a natural physiographic area and proceeded to treat it part-by-part as units comprising an ecological whole. The result was stimulating. A self-conscious animal ecology was born which, though frequently crude and inadequate, started a line of research and gave impetus to new activity. In short, a minor biological classic had appeared.

For this historical aspect one views this reprint of Shelford's book graciously in that it will permit younger students to get a copy for their shelves. It seems doubtful, however, if the book will prove of much professional value. This is unfortunate because it need not necessarily have been so. If, instead of merely reprinting the text, the author had modernized his views, corrected his errors and attempted to bring the volume in line with current standards the minor biological classic might have been elevated to a major one. As this was not done we can only feel grateful for partial favors and recommend the book in the same spirit that one might advise a youthful medico to purchase a first-edition of Gray's Anatomy.

GROWTH. *A Journal for Studies of Development and Increase. Volume I, Numbers 1–6, April, 1937.*
Subscriptions to be sent to the Secretary-Treasurer, Professor S. A. Courtis, School of Education, University of Michigan, Ann Arbor. $3.50 per volume of a minimum number of 400 pages (outside United States and Canada, $4.00). Single numbers 60 cents each. 9¾ x 6¾; 102; 1937.

This new journal is devoted to the dissemination of studies relating to any and

all aspects of growth phenomena regardless of the field of investigation. The first six numbers contain a paper by Davenport on a modification of the technique for measuring head features; one of a series of articles by Wetzel on a mathematico-physical theory of growth; a paper by Hammett and Schlumberger describing the effect of l-aspartic acid on the development of *Obelia*; a discussion by Northrup and Burr of an electro-dynamic theory of life; and a report by Goss and Asmundson on the glutathione and ascorbic acid concentration found in the carcasses of Barred Plymouth Rock and White Leghorn chickens of various ages.

In addition to its intention to cover the activities in a vast field, this new periodical has two distinctive features in editorial policy. In the first place, it is published by the coöperative efforts of authors and readers, the latter by subscription as is usual, the former by contributing to the cost of publication to a greater extent than is generally required. In the second place, the editors promise not to pass judgment on the merits of a work submitted. This policy may be instrumental in assuring the success of the new enterprise. Then again it may not.

KONGRESS FÜR SYNTHETISCHE LEBENSFORSCHUNG. *Verhandlungsbericht über die Aussprache zwischen Ärzten, Biologen, Psychologen und Philosophen in Marienbad vom 16. bis. 18. September 1936.*

Edited by M. Sihle and E. Utitz. J. G. Calve'sche Universitätsbuchhandlung, Prag. Kr. 10. $9\frac{1}{4}$ x 6; 208; 1937 (paper).

The volume gives the report of a congress for the promotion of medical synthesis held at Marienbad in 1936, and attended by prominent physicians, sociologists, psychologists, and philosophers. It contains essays on the following subjects: Synthetic thinking in research, by M. Sihle; Mnemistic biology and psychology, by E. Bleuler; Medical regulations in psychology, jurisprudence, sociology and political science, by E. Stransky; What service can philosophy render to the physician?, by O. Kraus; Brain pathology and personality, by H. Hoff; Life and spirit, by E. Utitz; The holistic investigation of biological phenomena, by L. v. Bertalanffy; Organ and organism under pharmacological influences, by E. Starkenstein; Psychotherapeutic syntheses in medicine, by Th. Bovet; General biology and medicine, by J. Bělehrádek; The present status of genetics in biology, by G. Wolff; The need for a world-view philosophy and philosophical psychology, on forms of regression, and psychotherapy, by M. Löwy.

The publication of this interesting report was made possible by the Josiah Macy, Jr. Foundation of New York, a subsidy that puts all theoretical biologists in its debt.

CONTRIBUTION A L'ÉTUDE DES RÉSERVES NATURELLES ET DES PARCS NATIONAUX.

By A. Aubreville, A. Barbey, E.-N. Barclay, C. Bressou, P. Chouard, J. et M.-L. Dufrénoy, F. Evrard, A. Feuillée-Billot, J. Fudakowski, H. Humbert, L. Joleaud, A. Joubert, L. Lavauden, P. Marié, G. Petit, P. de Peyerimhoff, E.-G. Racovitza, M. Tallon, C. Valois, V. Van Straelen and P. Vayssière. Paul Lechevalier, Paris. 100 francs. 10 x $6\frac{1}{2}$; 267 + 46 plates; 1937 (paper).

The Société de Biogéographie of Paris has set for itself the problem of finding the most reliable and most acceptable method of dealing with the problem of the conservation of natural things. The study, of which this monograph presents progress of findings to date, is a coöperative enterprise in that it represents the combined contributions of more than 20 workers.

The method used for making comparative studies of Natural Reserves and National Parks has been to find (1) the reason for the creation of the reserve or park; (2) the methods or techniques used in making the proposed reserve or park become a reality; (3) the results of periodic observations concerning the evolution of the fauna and flora of a reserve or park; and (4) the effect of immediate and surrounding influences upon the fauna and flora.

The work includes extensive studies of the parks and reserves of Europe, Asia, Africa and North America. The volume

is well illustrated with a number of fine photographs. There is a summary of topics discussed, but no index.

Desert Neighbors.

By Edith M. Patch and Carroll L. Fenton. Drawings by Carroll L. Fenton. The Macmillan Co., New York. $1.75. $7\frac{3}{4}$ x $6\frac{1}{4}$; vi + 170; 1937.

Here we have a work intended for children but which will appeal to adults as well—especially to the man who loves the desert well enough to want to spend his week ends on it, and to fry his eggs on the shovel over a fire of ocatilla wood, and sleep under the stars in the fragrance of the cresote bushes and the juniper trees, with the coyotes howling his lullaby and the quails singing his aubade—in short, it will appeal to all those who can say with the poet, "Desert, I love thy wan and dusty face."

The illustrations are well drawn, both of animals and of scenery. One in particular of a landscape in which an arroyo is the chief feature is so inviting that the reader will want to go walking in it, looking for petrified wood, fossil coral, and gypsum crystals, and make the acquaintance of Churca and Yodeler and Crota and Bannertail in their own homes, under the chollas and sahuaros and Biznagas.

A Graphic Summary of Physical Features and Land Utilization in the United States. *U. S. Department of Agriculture, Miscellaneous Publication No. 260.*

By O. E. Baker. Government Printing Office, Washington. 10 cents. $9\frac{1}{4}$ x 6; 57; 1937 (paper).

This pamphlet, arranged in two sections, consists of seventy maps and graphs based on census reports and the annual estimates of the Bureau of Agricultural Economics. The graphs in the first section deal with the physical conditions in the various states, including temperature, duration of frost season, moisture, topography, and the various kinds of soil in each section which are influenced in their development by climate and natural vegetation. The second section shows how the land is being used in each state. Maps indicate crop, forest, and pasture land, dry land that has been irrigated, and swamp land that has been drained. Crop failures as well as those harvested are indicated.

There is a complete index.

Concord River. *Selections from the Journals of William Brewster.*

Edited by Smith O. Dexter. Illustrated by Frank W. Benson. Harvard University Press, Cambridge. $3.50. 9 x $6\frac{1}{4}$; vii + 259 + 12 plates; 1937.

This book is actually a sequel to *October Farm*—a diary of daily wanderings around a country home. It includes selections Mr. Dexter made from Brewster's diaries that were not included in the first publication.

Wherever Brewster went, whether merely a few steps from his door or for an overnight trip in his decked canoe, he saw birds before all else. These he described in natural and simple language that brings the animal life around Concord River into a living picture. Although the selections in this book are of the same type as those in *October Farm*, Mr. Benson's twelve excellent bird studies have added a new touch to *Concord River*. Three of these are in color, one is an etching, and eight are charmingly done in black and white wash. All stand out as different, realistic and exceptionally artistic.

There is a foreword by Thomas Barbour and a detailed index.

Mitogenetic Analysis of the Excitation of the Nervous System.

By A. G. Gurwitsch. N. V. Noord-Hollandsche Uitgeversmaatschappij, Amsterdam. F. 3.75 (paper); F. 4.75 (cloth). $9\frac{7}{8}$ x $6\frac{1}{4}$; 114; 1937.

This book is a theoretical outline of results obtained on work on mitogenetic phenomena of the nervous system. Part I deals with the radiation of the elements of the nervous system; Part II with the

effects of mitogenetic radiation on the nervous system. Some very interesting results are obtained, which are interpreted to show that the present concepts of nerve stimulation must be altered. It is claimed that spectrum analysis reveals the qualitative variability of the states of excitation of the nerve fiber and that its results refute the all-or-none law. Many other new ideas are advanced.

TESTS IN BIOLOGY.
By Frederick L. Fitzpatrick. Houghton Mifflin Co., Boston. 40 cents. 11 x 8½; 32; 1937 (paper).
These tests will certainly be appreciated by lazy teachers and by those who are overworked and have classes larger than they can handle properly. For the most part they consist of true-or-false questions, sentences with words to be filled in, and sentences where the student is to choose which of several indicated words makes a true statement. This makes it extremely easy to grade the papers, particularly since the author is kind enough to supply the teacher with a set of the "correct" answers. We should not be inclined to give the author a grade of 100 per cent. Many of the questions are so worded that no answer in the required form can be strictly correct and some others are petty if not positively silly.

EFFECT OF TEMPERATURE, HUMIDITY, AND OTHER FACTORS ON HATCH OF HENS' EGGS AND ON ENERGY METABOLISM OF CHICK EMBRYOS. *U. S. Department of Agriculture, Technical Bulletin No. 553.*
By H. G. Barott. Government Printing Office, Washington. 10 cents. 9 x 5⅞; 45; 1937 (paper).
Over a period of four years the author conducted a series of careful experiments for the purpose of obtaining the optimum conditions for the hatch of hens' eggs and normal development of embryos. These conditions of temperature, humidity, and of oxygen and carbon dioxide content are presented to poultry raisers in an effort to reduce "an annual monetary loss of at least $14,000,000". After a brief review of other literature, there is a description, with photographs, of the respiration calorimeter used in the author's own experiments. Results are well depicted by frequent graphs and tables. The bulletin contains a table of contents and summary, but is not indexed.

BUGS, BIRDS AND BLIZZARDS IN THE YELLOWSTONE.
By Harlow B. Mills. Collegiate Press, Ames, Iowa. 50 cents. 9 x 6; vii + 76; 1937 (paper).
A naturalist here reveals to his audience the high spots among his recollections of a year spent on the range in the Yellowstone National Park. The birds of this forest reserve were a special delight to the author but his descriptions are mostly too fragmentary to convey his emotions to the reader. Most interesting is the story of a skiing trip taken around the "loop" of the park when deep snows and blizzards have made the country an isolated wilderness, but a place of beauty and charm in which some forms of wild life are still in evidence.

ECOLOGY IN TOWN AND CLASSROOM.
By R. Bracher. J. W. Arrowsmith, London. 2s. 6d. net. 7¼ x 4¾; 96; 1937.
This is a companion volume to *Field Studies in Ecology*. Whereas that book dealt with natural types of flora this deals with what are called artificial, since they have in one way or another been influenced by human beings. Studies were made and lists compiled of plants found in the following habitats: (a) streets, (b) waste ground (building sites, neglected gardens, rubbish dumps), (c) coal tips (with other artificial substrata), (d) fresh water communities (ditches, etc.), (e) tidal river banks and dockland.

ÖKOLOGIE ALS WISSENSCHAFT VON DER NATUR *oder Biologische Raumforschung. Bios, Band VII.*

By Karl Friederichs. Johann Ambrosius Barth, Leipzig. RM. 8. $9\frac{1}{4}$ x 6; vii + 108; 1937 (paper).

The term ecology as used in this book does not denote a subdivision of biology, but a unified bio-centric science of Nature as a Whole, a Cosmos. The author makes a strong plea for a synthetic (organic, dynamic, holistic) study and interpretation of natural phenomena and their relations, man included, yielding not only knowledge but wisdom, and seeing in Nature a revelation of God.

General Biology. *Revised Edition.*

By S. J. Holmes. Harcourt, Brace and Co., New York. $3.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; vii + 467; 1937.

This entirely rewritten edition of a well and favorably known text has a new chapter on elementary chemistry, useful to beginning students of biology, added at the beginning.

HUMAN BIOLOGY

Damien the Leper.

By John Farrow. Sheed and Ward, New York. $2.50. $8\frac{1}{4}$ x $5\frac{1}{2}$; xx + 230; 1937.

On the shore near the Kalaupapa landing in Molokai there stands a granite shaft bearing the inscription "Damien de Veuster. . . . Greater love hath no man than this, that a man lay down his life for his friends"—a strangely inadequate tribute to one whose entire life, like that of the original Author of these words, was a demonstration of a still greater love, one that impels a man to lay down his life for those who by no stretch of the imagination can be called his friends, yes, even for his enemies.

Father Damien really laid down his life sixteen years before physical death finally overtook him, for when he entered on his ministry on Molokai he knew that he would never be able legally to leave the island, and that it would be only a question of time until even his vigorous frame succumbed to what in ancient Egypt was known as the death before death. And at this time none of the unfortunate victims with whom he threw in his lot could be called his friends, for none of them had ever heard of him—in fact, his life was attempted by voodoo practitioners, and the lepers themselves arose in open revolt against him. But Damien was not one to turn back after having put his hand to the plough, with the result that when he was finally called to his reward as an old man at forty-nine, blind, paralyzed, and nauseated by the stench of his own decomposing flesh, he left behind him the devotion and love of his parishioners, the gratitude and admiration of intelligent people everywhere, and a reputation that defies the flight of time. He was like the bush that Moses saw on the desert of Midian—burning but not consumed, and the historian who contemplates his biography feels as Moses did, that he is standing on holy ground. Like Lincoln at Gettysburg, he feels the futility of words to add anything to deeds, and that about all he can do is to recount the facts.

In this instance, the author has not been altogether happy in the choice of facts he has chosen to emphasize. He is a member of the same denomination as Father Damien, and is quite justly proud of the record of his co-religionist. But this tends to make him somewhat unfair to those who adhere to creeds other than his own. He seems resentful of the fact that when the first permanent Roman missionaries landed on the islands under the protection of the guns of the French navy they found the field occupied by New England Protestants, who had been for about a decade unobtrusively but effectively laying the foundations on which later missionaries were to build. He also points out that the mob that revolted against Damien was led by Calvinists and Mormons, a statement perhaps true but certainly not necessary. Finally, he repeats the Hyde letter, which should have been consigned to oblivion long ago, and probably would have been, had a copy of it not fallen into the hands of Robert Louis Stevenson, a member of the same denomination as Mr. Hyde, who felt moved to reply to it—as if Damien needed vindication! The result has been the perpetuation of an unsavory controversy that would better have been forgotten. One

feels that if Hyde had been a Romanist Mr. Farrow would have passed him over in the silence he merits.

Apart from these details the book deserves a great deal of praise, for it is a scholarly piece of work, as the great number of acknowledgments at the beginning and the extensive bibliography at the end bear witness. Two unexpected chapters add tremendously to the force of the book—one is a history of the disease since the earliest mention of its occurrence in the valleys of the Nile and Euphrates to the present day, and the other a biographical sketch of Brother Dutton, Damien's able assistant, the revelations in the latter chapter being somewhat startling, in view of the fact that Brother Dutton is still living, or was until recently.

The story is well told, and the literary style of the writer grips the reader and holds his attention till the book is finished. Perhaps the most dramatic incident in the life of the hero is the confession that he made on the open ocean from his canoe to a priest standing in the stern of a passing steamer. The captain had stopped the ship for the occasion, but he had been given strict orders not to allow anyone to leave or board the ship at Molokai. Under these circumstances it was impossible to keep the confessional secret, but it is to the credit of the crew and the passengers that not one of them ever divulged anything which they might have heard.

The book is provided with a complete index of four pages, and a pen and ink drawing of Father Damien by Jean Charlot for frontispiece.

A Brief Rule to Guide the Common-People of New England. *How to Order Themselves and Theirs in the Small Pocks, or Measles. Publications of the Institute of the History of Medicine, The Johns Hopkins University, Fourth Series, Bibliotheca Medica Americana, Volume I.*

By Thomas Thatcher. Facsimile Reproductions of the Three Known Editions with an Introductory Note by Henry R. Viets. The Johns Hopkins Press, Baltimore. \$1.50. $7\frac{3}{4} \times 5\frac{1}{4}$; liv + 16; 1937.

A Discourse upon the Institution of Medical Schools in America. *Publications of the Institute of the History of Medicine, The Johns Hopkins University, Fourth Series, Bibliotheca Medica Americana, Volume II.*

By John Morgan. With an Introduction by Abraham Flexner. The Johns Hopkins Press, Baltimore. \$2.00. $7\frac{3}{4} \times 5\frac{1}{4}$; vii + xxviii + 63; 1937.

Adaptation in Pathological Processes. *Publications of the Institute of the History of Medicine, The Johns Hopkins University, Fourth Series, Bibliotheca Medica Americana, Volume III.*

By William H. Welch. With an Intro- by Simon Flexner. The Johns Hopkins Press, Baltimore. \$1.50. $7\frac{3}{4} \times 5\frac{1}{4}$; xi + 58; 1937.

The first of these volumes (of a new series of the publications of the Institute of the History of Medicine of the Johns Hopkins University, *Bibliotheca Medica Americana*), is a facsimile reproduction of the earliest medical document printed in America, north of Mexico. In a frontier community such as seventeenth century New England where physicians were few, the clergy often ministered to their parishioners' bodily as well as spiritual ills. When therefore an epidemic of smallpox broke out in Massachusetts in 1677, the Reverend Thomas Thatcher, minister of the Old South Church, prepared a broadside, based upon Sydenham, describing the symptoms and treatment of the disease. This was reprinted during the epidemic of 1702 and again in 1721–22. These reprints are here reproduced for the first time. A brief life of Thatcher, a discussion of smallpox and other epidemic diseases in England and New England, and a bibliographic discussion of the three editions are also included.

The second volume is a facsimile reproduction of the address in which the first professor of medicine in this country set forth his views on the proper organization of a medical school. In it he emphasized the importance of a thorough grounding of the medical students in anatomy, chemistry and physiology, as well as the need of hospital facilities for their clinical instruction. He realized also that the system then customary in this country, by

which the same person prescribed and compounded medicines, was less efficient than a system in which the physician and the pharmacist might each have opportunity to develop his specialized knowledge.

The third volume is a reprint of Welch's presidential address of 1897 before the Congress of American Physicians and Surgeons. The basis of pathological adaptations, Welch pointed out, is to be sought in the physiological properties of cells. Thus the compensatory hypertrophy of the heart is a special case of the hypertrophy of a muscle required to perform prolonged extra work, "a capacity which has abundant opportunities for exercise in normal life, when the influence of natural selection and other factors of evolution can exert their full power."

NEW ENGLAND AND NEW AMERICA *and Other Writings of J. Lionel Tayler. With Memoir and Tributes.*

Edited by F. H. Hayward. Williams and Norgate, London. 10s. 6d. net. $9\frac{7}{8}$ x $6\frac{1}{4}$; viii + 335; 1933.

THE STUDY OF INDIVIDUALS (*Science*).

By J. Lionel Tayler. J. W. Sparks, London. 5s. 6d. $8\frac{1}{2}$ x $5\frac{1}{2}$; ix + 241; 1936.

J. Lionel Tayler died in 1930 at age 56 after a career which, if not extraordinary, was at least unusual. He was first trained as a physician but later acquired an interest in theology and eventually became a Unitarian minister. Scientific popularization was his *forte* and for many years public lecturing constituted his major activity. He was a M.R.C.S., L.R.C.P., M. San. I., and London University Extension Lecturer in Biology and Sociology. Although he wrote a number of books he died practically unknown in science or literature. Apparently his lectures were more effective because the friends he acquired from them have formed a committee to honor his memory and by republishing his works hope to arouse interest in his ideas.

The first of the two books mentioned here contains a number of exemplars of Tayler's publications. There are selected passages from some of his writings which will interest primarily persons of a religious frame of mind. The title of the book derives from a philosophical essay in which Tayler praises the cultural traditions of New England. In this volume is also presented an attempt at fiction. It is a sketch of the trials of a man who has temporarily embraced vegetarianism and modifies his daily life to be wholly consistent with that idea. Doctor Tayler clearly was not meant to be a fiction writer.

In the second book is detected the germ of an idea which, if properly developed, would have given Tayler a high ranking among human biologists. In opposition to the methodology of the sociologists Tayler justly believes that the study of social phenomena must be based on a definite knowledge of individual biology. Therefore as a starting point he considers the differences in somatologic constitution. He proceeds to a classification of individual social behavior in terms of anatomical and physiologic personality, then discusses variation in mental characteristics and finally the processes of physical and mental development. For a popular piece of work, this book is neither sufficiently simple nor interesting. As a scientific study it lacks clearness, is too superficial, and demonstrates that the author could not entirely disassociate himself from his religious calling.

THE AMERINDIANS. *From Acuera to Sitting Bull, from Donnacona to Big Bear.*

By Donald M. McNicol. Frederick A. Stokes Co., New York. \$2.50. 8 x $5\frac{1}{2}$; xix + 341; 1937.

The complete story of the Amerind (or Amerindian, as this author prefers to call him) has never been told, and probably never will be, since except for the astronomic symbols on the Maya megaliths and the undecipherable quipus of South America, he kept no records. The first authentic event in Amerind history is the arrival of the paleface, and from Gaspar Cortereal in the first year of the sixteenth century down to the eviction of the Palatingwas in the twentieth, this history has been a systematic development of exploitation, expropriation, and dishonesty on the part of the white man, with only a

few notable exceptions, such as Bartolomeo las Casas in one continent and William Penn in another.

The tendency of historians of the past has been to dwell on the barbarity of Indian character, to emphasize the tomahawk rather than the pipe of peace. Within recent years the pendulum has swung to the other extreme, with the result that the Indian is now unduly idealized and exalted. As is so often the case, both views are correct in what they affirm and wrong in what they deny. Good and bad people exist among the Indians just as among the whites, and probably in about the same proportion. But there was this important difference between the two races—a difference in population density. At the beginning of European colonization there were probably not over five thousand Indians in Pennsylvania, or a million in all North America. With an abundance of land for all, there was no need for land titles—the Indian had no conception of the private ownership of land. When he sold land, he sold the right to use it and to enjoy the fruits of it insofar as that might be necessary. Even under the land laws obtaining in Europe, in order to clear title to real estate it would have been necessary to obtain a quit claim deed from every Indian in the western hemisphere—and it was easier to shoot them.

The true story of the resulting relations and contacts of the two races can be told impartially by one who knows the Indians as he knows the whites—and there are but few such writers. Mr. McNicol is one of them. He has succeeded in portraying the view point of both sides with equal fidelity, and it is to be hoped that other supplementary works will soon issue from his pen, as the present volume with its index of only five pages, is too small to tell the whole story.

Primitive Intelligence and Environment.

By S. D. Porteus. The Macmillan Co., New York. $3.00. $8\frac{1}{2}$ x $5\frac{5}{8}$; viii + 325; 1937.

Many sociologists and psychologists have asserted that the intellectual development of a people is related to the physical environment which it inhabits, in the sense that a harsh environment will have a repressive effect while a pleasant environment will induce a favorable reaction. In order to test the validity of this assertion, Porteus journeyed through the bleakest sections of Australia, the central and north western parts and examined with his maze test 120 aborigines. He then proceeded to the Kalahari desert and there examined 25 Bushmen. Porteus reasoned that if between two primitive peoples that live in a similar harsh environment there is a difference in mental abilities, this difference cannot be attributed to the physical environment but must be considered as due to dissimilarity in innate mental equipment. The results of his survey indicate that the Australian aborigines, whose country is equally as unfavorable if not more so than that of the Bushmen, have distinctly better scores than the Bushmen. A careful examination of his data leads one to doubt the significance of these results. The Australian aborigines had had some schooling at the hands of the whites while the Bushmen had not and the figures presented distinctly show that the Australian subjects who had received no schooling obtained scores inferior to those of the same race who had received instruction. Nevertheless, Porteus concludes, rather tentatively, that in planning ability the Australians are superior while the Bushmen have the advantage in imaginative skill. In the mastery and use of the environment, Porteus believes that the two races are equal.

The presentation of this investigation is not impressive. Only about one-third of the book is dedicated to a description of the results and Porteus devotes a good portion of this section to polemicize with those who have criticized him. The remaining two-thirds of the book contains an account of the author's journey through Australia and South Africa. Porteus justifies this curious arrangement by pleading the need for a thorough description of the habitats of the races examined. However, the account partakes more of the nature of an adventure tale than of an ethnographic or ecologic study.

Studies in the Science of Society. *Presented to Albert Galloway Keller in Celebration of His Completion of Thirty Years as Professor of the Science of Society in Yale University.* *Edited by George P. Murdock. Yale University Press, New Haven; Oxford University Press, London.* $6.00. 9 x 6; xxii + 555 + 2 folding charts; 1937.

Everyone of the 26 contributors to this *Festschrift* acquired his doctorate for work done under Professor Keller. Through him, each has been imbued with the ideas of the greatest American sociologist, William Graham Sumner, whose influence is noticeably evident in the majority of the articles.

About one-half of these deal with some aspect of the folkways of contemporary society, the remainder discuss primitive or historical peoples. Included in the former group of articles is an excellent discussion of unemployment benefit by Bakke, and a paper by Davie who presents strong evidence against Burgess's theory of the pattern of urban growth. Among the articles that treat of primitive social behavior patterns there are two, one by Murdoch and the other by Simmons, which deserve special mention. These represent an attempt, somewhat feeble it is true, but nevertheless an attempt, to analyze the concomitance and covariation of social traits by means of statistical methods. These authors have sought to measure for a series of populations the degree of association between given traits, for example, between private property of land and agricultural stage of economy. For this purpose they have utilized Yule's association coefficient, uncritically accepting the results. From a methodological standpoint the procedure is not entirely satisfactory, but a study of these papers should indicate to the general sociologist the need of a finer statistical technique to evaluate generalizations regarding social phenomena.

The main characteristic of all this series of articles is that the discussions are always objective and the conclusions usually based on sufficient data. In comparison with the general run of sociologic literature they manifest a very high standard of scientific quality.

Early Medieval Medicine *with Special Reference to France and Chartres. The Hideyo Noguchi Lectures. Publications of the Institute of the History of Medicine, The Johns Hopkins University, Third Series, Volume III.* *By Loren C. MacKinney. The Johns Hopkins Press, Baltimore.* $2.75. 7¾ x 5¼; 247; 1937.

The three lectures deal respectively with The Dark Age concept and early medieval medicine; Medicine in Merovignian and Carolignian France; and Medical progress at Chartres in the tenth and eleventh centuries. The first of these, a survey of some of the general aspects of medical practice prevailing in Western Europe, develops the view now gaining acceptance by historians of medicine and other fields of human enterprise, that the centuries following the collapse of the Roman Empire have been mislabelled "dark", not because of lack of cultural progress during that era, but because of insufficient evidence so far obtained by modern scholars as to the achievements of the times. The second lecture is concerned with France (approximately the Gaul of Roman times) and the third is restricted to a single locality, the cathedral school at Chartres.

The most frequently quoted sources on the civilization of the Merovignian period are the works of Bishop Gregory (died 593) and other religious writers who placed disease as a consequence of sin and its treatment as a matter of divine intervention. However, the existence of ordinary physicians is evident from the frequency with which they are mentioned, but, not being looked upon with favor by the clergy, detailed records of their enterprises are lacking. During the Carolignian period the study of medicine became a part of curriculum in the monasteries, gradually progressing to an appreciation of classical medical science and intelligent practice of the results of experience.

The author concludes that: "Western medicine, which in the sixth century was well toward the bottom of the ladder, by 1100 had made noteworthy progress. Far from being stagnant and unproductive, the 'dark age' was an era of vigorous activity."

Documentary notes, a series of plates, and an index are provided.

AMERICANS IN PROCESS. *A Study of Our Citizens of Oriental Ancestry.*
By William C. Smith. Introduction by Romanzo Adams. Edwards Brothers, Ann Arbor, Michigan. $3.00. 8 x 5¼; xv + 359; 1937.

This is an interesting discussion of the problems that confront the American born Oriental in Hawaii and in the western states of continental United States. Because of his distinctive physical appearance, the attempts of the American Chinese or Japanese to participate actively in the social and economic life of his native land have been in part successfully opposed by those whites who have delusions of racial superiority or fear of economic competition. In Hawaii, due to the extensive intermingling of many races, the situation has on the whole been much more favorable. But even there an undercurrent of prejudice prevents the Orientals from exchanging freely the status of plantation workers, the prevailing occupation of their fathers, for positions in the clerical or professional fields. So much is this the case that the author advises ameliorating the conditions of the plantations in order that the American Oriental will be satisfied to stay there. In California and other western states the situation of the American Oriental is even worse. It is well known that they are discriminated against in schools and in economic activities, consequently in some a feeling of suspicion and distrust towards whites has developed together with a pessimistic outlook for the future. However, the author believes that here as well as in Hawaii, time will eventually bring about a change in the attitudes of both the Orientals and the whites and will lead to a satisfactory adjustment of their relations. The book is well written and documented and contains useful information regarding the demography of the American Oriental.

RASSENBIOLOGISCHE UNTERSUCHUNGEN *an dem hygienischen Institut der medizinischen Fakultät zu Kanazawa. Numbers 1, 2, 3, and 4. Under the direction of Prof. Dr. Y. Koya.*
Edited by Y. Koya. Kanazawa, Japan. 10⅛ x 7¼; No. 1, 4 + 434; No. 2, 3 + 448 + 2 plates; No. 3, 4 + 370; No. 4, 7 + 276 + 3 plates; Nos. 1 and 2, 1936; Nos. 3 and 4, 1937 (paper).

These beautifully printed and edited volumes present the results of a perfectly stupendous amount of biometrical and statistical research in the general field of human biology. The topics in which Prof. Koya and his collaborators are evidently most particularly interested are physical anthropology of the living, human genetics, growth of children, fertility and population problems, and the social aspects of human biology.

Unfortunately for most readers outside of the authors' own country the contents are presented in the Japanese language. For most of the included papers, however, author summaries in German are given. Also in the body of the Japanese text intelligible clues to the meaning of the figures in the numerous tables are generously interspersed.

Space is not available even to list the titles of the contents of these four solid volumes, or much less to give any account of the interesting results achieved. But we can say, and it gives great satisfaction to do so, that no one interested in any aspect of human biology can afford to neglect studying the mass of first-rate quantitative research that is coming out of Prof. Koya's Institute. It makes the impression of being very thoroughly and carefully done, and deals with interesting and significant problems. We shall hope to notice in more detail in these columns future volumes in the series as they appear. In the meantime we wish to congratulate Prof. Koya on the fine work he is directing and producing.

ZUR OSTEOLOGIE DER LAPPEN. *Instituttet for Sammenlignende Kulturforskning Serie B: Skrifter XVIII. Two volumes.*
By K. E. Schreiner. H. Aschehoug and Co. (W. Nygaard), Oslo; Harvard University Press, Cambridge. Kr. 95 for two volumes; Vol. 1, $7.50; Vol. 2, $25.60. 11½ x 9; Vol. 1, 294; Vol. 2, 74 + 278 charts + 101 plates; Vol. 1, 1935, Vol. 2, 1931.

The skulls and partial or complete skeleton remains of 582 Lapps compose the material for this fine anthropological study. The material, now in possession of the University of Oslo, was subjected to the following investigation: Skull measurements and calculation of skull capacities and indices, cranial and facial features described and measured; descriptions of sutures and variations found in them; measurements on 101 children's skulls with calculation of skull capacity and indices; measurements of trunk bones and long bones and various skeletal proportions.

The results of the investigations show, as previous workers have also found, the Lapps to be a mixed race, fitting in between the Mongoloid and Alpine race stocks. It is possible that the Lapps belong to a prehistoric race who before the last ice age lived on the edge of the Altai, whence some wandered to Mongolia, some to Western Europe, while the ancestors of the Lapps went north to Finland. However, as the author points out, it is very difficult to find any very ancient skeletal remains of the Lapps, so it is impossible to come to any definite conclusion about them. Volume 2 of this book contains tables of measurements, skull contours, and photographs. There is a bibliography and an index.

SPORT: Gli Uomini e le Macchine. *Studio Biometrico dello Sport e degli Sportivi.*

By Alfredo Niceforo and Dino Vampa Societa Editrice del "Foro Italiano", Rome. $9\frac{1}{2} \times 6\frac{1}{4}$; xii + 357; 1937 (paper).

A number of years ago Niceforo advanced the idea that the group of human activities classified as sports or athletics would be a fertile field for the application of statistical technique and an additional source of information regarding human variability. The idea is further developed in the first part of this book in which Niceforo outlines the objectives of sport biometrics. In the main, these are to measure (a) the variation in individual productivity, (b) the relation of physical and functional characteristics to athletic ability, (c) the correlation of athletic abilities in several sports, (d) the evolution and progress in the results and in the popularity of each type of sports. The technique to be adopted in such investigations is well illustrated by examples drawn from the records of races, games and other athletic meets.

In the second part of this book, Vampa makes a practical application of Niceforo's ideas principally in the study of 108 amateur athletes of Olympic calibre. A number of interesting facts are brought out; for example, to mention but two, he notes that there is high correlation of individual proficiency in several sports and that the secular changes in speed records can be represented by a logistic type of curve.

This work, based on sound methodologic principles, should serve to stimulate interest in the field of sport as a source of material for the study of human variability. In this country much could be done on this subject since the results of meets and games are easily available and sufficiently accurate.

HISTORY OF THE ARABS.

By Philip K. Hitti. The Macmillan Co., New York. $10.50. $8\frac{5}{8} \times 5\frac{3}{4}$; xvii + 767; 1937

Professor Hitti, of Princeton University, a native of Mt. Lebanon and educated in the University of Beirut, gives us in this book a most valuable history of the Arabians and the Arabic speaking peoples from the earliest times to the Ottoman conquest of the 16th century. It is based not only on the works of other scholars, but on a first hand and independent study of the original (Arabic) sources. It is accurate in scholarship, lucid and direct in style, appealing to the cultivated layman as well as to the scholar by its touches of humor and its breadth of human sympathy. The author deals with every phase of Arab history, science, civilization and culture, paying particular attention to the various ethnic factors.

What we therefore call "Arab civilization" was Arabian neither in its origins and fundamental structure nor in its principal ethnic aspects. The purely Arabian contribution in it was in the linguistic and to a certain extent in the religious fields. Through-

out the whole period of the caliphate the Syrians, the Persians, the Egyptians and others, as Moslem converts or as Christians and Jews, were the foremost bearers of the torch of enlightenment and learning just as the subjugated Greeks were in their relation to the victorious Romans. The Arab Islamic civilization was at bottom the Hellenized Aramaic and the Iranian civilizations as developed under the ægis of the caliphate and expressed through the medium of the Arabic tongue.

The author has provided an index, genealogical tables, maps, illustrations, and bibliographic annotations. This is a book of the very first rank of importance.

A Black Civilization. *A Social Study of an Australian Tribe.*

By W. Lloyd Warner. Harper and Brothers, New York. \$5.00. $8\frac{1}{2} \times 5\frac{1}{2}$; xviii + 594 + 9 plates; 1937.

In this book Professor Warner gives a detailed description and analysis of the kinship structure, age grading, technology, warfare, magic, medicine, totemism, and mortuary rites of the Murngin, a tropical Australian people. As the tropical Australian tribes have hitherto not been studied as intensively as those of other parts of the continent, this work is a valuable addition to our knowledge of a people of great interest to the anthropologist.

During his first eight or nine months among the Murngin Warner was convinced, as were Spencer and Gillen for the tribes which they had studied, that the people had no knowledge of physiological conception. However, when he inquired directly of certain old men just what the semen did when it entered the uterus of a woman, "they all looked at me with much contempt for my ignorance and informed me that 'that was what made babies.'" His previous failure to elicit this information was due, he suggests, to the greater interest of the savage in ritual and myth, which determine the child's place in the social life of the people, than in the prosaic details of physiology. As far-reaching sociological and anthropological theories have been based on the supposed ignorance of many primitive peoples of physiological conception, he suggests that such speculations be deferred until their factual basis is further investigated.

Back in the Stone Age. *The Natives of Central Australia.*

By Charles Chewings. Angus and Robertson, Sydney. \$2.50. $8\frac{3}{4} \times 6\frac{1}{8}$; xviii + 161 + 23 plates; 1936.

The past year has seen a multiplicity of books of this type, and undoubtedly, many of them have met disapproval because of the monotonous repetition and duplication of material already written. The present volume, however, does not suffer this handicap, because essentially it is the result of long, patient, and acute observation, and not merely the record of happenings of a two-months adventure trip through uncivilized lands.

Dr. Chewings has spent over 50 years in Central Australia, living among the natives and studying their personal and tribal life. He has so dealt with them that he has gained their confidence, and has been able to study at close range their characteristics, habits, beliefs and superstitions. He has also studied the tools and weapons of the different tribes, and has found much interest in observing the effect of the coming of the white man on the life of the native.

The book is written in simple style and language, and is intended for "the man in the streets." The 23 full page plates, which depict the landscape and the peoples of Central Australia, add charm to this enlightening piece of work.

Civilization *as told to Florence Drake.*

By Thomas Wildcat Alford. University of Oklahoma Press, Norman. \$2.50. 9×6; xiii + 203 + 6 plates; 1936.

Thomas Wildcat Alford, born of parents of the Absentee Shawnee tribe of Indians in 1860, relates the story of his life and of his tribe during the period from which the influence of the white race first began to be felt, up to the present when the so-called civilization of the white man has superceded almost to annihilation the old tribal mode of life.

Early recollections reveal pictures of Shawnee home and tribal life—of child life and parent-child relationship, Indian food, ceremonials, tribal government and many other of the ways of the Indian. Then

comes the picture of the transition from the old cultural life to the ways of the white man. It was not an easy transition and many of the older Shawnees fought bitterly against the intrusion of the white settlers and the ever increasing dominance of their civilization.

Alford, a great grandson of the famed Tecumseh, was trained from early youth for the eventual position of chief of his tribe, but during the course of his education at Hampton Institute he renounced the faith of his fathers and so became ineligible for the desired honor. His life has been devoted, however, to work among his people as an educator, interpreter and leader.

TWENTY-FIFTH ANNIVERSARY STUDIES. *Volume I. Publications of the Philadephia Anthropological Society.*

Edited by D. S. Davidson. University of Pennsylvania Press, Philadelphia; Oxford University Press, London. \$2.50. 9 x 6; vi + 235; 1937.

Eighteen articles dealing especially with problems of ethnology and archeology are contained in this volume. The majority of them are short and deal with some particular controversial point. Among the ethnological papers, those deserving special mention include Hallowell's concise but comprehensive description of the frequency of cross-cousin marriages among the aborigines of the Lake Winnipeg region, the influence of this practice on kinship terminology, and the modification of the *mores* due to contact with the Indians and the whites. Another paper of special value is that by Davidson on the question of the relations between Tasmanian and Australian cultures. Davidson lists in a rather convincing manner the principal evidence which leads him to agree with the theory that the Tasmanians originated in Australia. An article by Speck on the medical therapeutic practices of the Catawba Indians and one by Spencer on Fijian dream interpretations would be of great interest to the human biologist if the subject concerned in the articles had been treated in a more complete fashion. With some exceptions this is the general fault of the series of papers contained in this volume, they are well written and documented, but somewhat superficial and not sufficiently informative.

HALF-CAST.

By Cedric Dover. With a Preface on Prejudices by Lancelot Hogben. Martin Secker and Warburg, Ltd., London. 10s. 6d. $8\frac{5}{8}$ x $5\frac{1}{4}$; 324; 1937.

The author of this book, himself a half-caste, feels that the problem of race-intermixture has passed beyond the point of being considered only as isolated, local cases. He believes that it is a general, world-wide problem—and, moreover, that it has been such since the beginnings of civilization and possibly even before that.

In this volume the problem has been considered from a rational point of view which the author hopes may prove suggestive to an intelligent and serious minority. Because he is a scientist Mr. Dover has been able to achieve the almost-impossible: he has succeeded in seeing not only the trees, but the woods as well. However, although the facts presented are impartial, the presentation is delicately flavored with contempt for the irrational smugness and prejudice of the white races in general and various peoples in particular, thus enlivening the discussion.

LE FINNMARKIEN. *Les Origines de la Civilisation dans l'Extrême-Nord de l'Europe. Instituttet for Sammenlignende Kulturforskning Serie B: Skrifter XXXII.*

By Johs. Bøe and A. Nummendal. H. Aschehoug and Co. (W. Nygaard), Oslo; Harvard University Press, Cambridge. N. Kr. 25; \$7.80. 12 x $9\frac{1}{4}$; [9] + 263 + 104 plates, 1937 (paper).

During the past year, nearly 60 stations of prehistoric industry have been found and worked in Finmark, the extreme northern tip of Norway. The finds, including very crude tools and weapons, none of which show any evidences of polishing, point very strongly to a unique and substantial early civilization in the extreme north of Europe. The complete lack of fauna from

the stations and the much disputed age of the floral fossils make it very difficult to date the "Finmarkien" either in terms of relative or absolute chronology.

The latter half of the book is made up of 104 plates illustrating 12 station sites and 500 different stone weapons and tools. The bibliography of 188 references does not include all the citations to literature made in the text. The volume contains a brief "Table de Matières", but no index.

GLOBE TROTTING WITH A SURGEON.
By Alexander H. Peacock. Lowman and Hanford Co., Seattle, Washington. $2.50. $8\frac{1}{2}$ x $5\frac{3}{4}$; 276; 1936.

A new travel story of a trip around the world will always find interested readers, since each globe trotter sees through different eyes and records his views in his own individual style. This is the story of a four months tour made over sea and land, starting from San Francisco and touching Hawaii, Japan, China, the Philippines, Singapore, Ceylon, Bombay, Cairo, Italy, France, England and back across the United States. One is impressed from this account with the great haste with which one—on a tour of this time duration—must continually dash around during those days on land in order to see even a small fraction of the sights at hand. Do not be deceived by the title into believing that medical interests are of any special significance on this trip. The doctor was on vacation. The author's special interest it would seem, was in the art treasures of Italy.

THE DEVELOPMENT OF EUGENIC POLICIES. *Scientific Backgrounds for a New Orientation of Eugenics.*
American Eugenics Society, 50 West 50th Street, New York. $8\frac{1}{4}$ x 5; 23; 1937 (paper).

CONFERENCES ON THE RELATION OF EUGENICS TO THE FIELDS OF RECREATION—NURSING—EDUCATION AND MEDICINE. *Reported at the Meeting of the American Eugenics Society May 14, 1937.*
American Eugenics Society, 50 West 50th Street, New York. $8\frac{1}{4}$ x 5; 23; 1937 (paper).

The first of these pamphlets was prepared by the directors of the American Eugenics Society for the members of the conferences held, and to be held, in 1937 and 1938. It deals briefly with such general considerations as the decline in births, the proportion of large families, and the agreement on desirable qualities as well as with the nature of eugenic policies, positive and negative eugenics, etc.

The second pamphlet contains brief reports written by members of the conferences held on the subjects listed.

THE LABOR SUPPLY IN THE UNITED STATES. *Occupational Statistics of the 1930 Census Tabulated by Class of Work and Industry, as Well as by Sex, Race, and Age Groups. Revised June 1937.*
By W. S. Woytinsky. Committee on Social Security, 726 Jackson Place, Washington, D. C. $1.50. $10\frac{3}{4}$ x $8\frac{1}{2}$; 131; 1937 (paper).

The author has rearranged the occupational statistics of the 1930 census in order to obtain for each class of industry data on the number, age, sex, race(color) and nativity of the persons affected by the provisions of the Social Security Act. This rearrangement necessitated reclassification of the industries and of the classes of work, and separation between independent and salaried workers, between professional persons, managers and other white-collar employees, and between skilled, semi-skilled, unskilled and service workers. The method used is explained in detail and the results presented in 10 tables and 8 charts. On the basis of the 1930 census and assuming certain plausible conditions, it is estimated the 26.3 million persons are affected by the Social Security Act. The conclusions to be drawn from these figures will be presented by the author in a future publication.

THE ETHNOGRAPHY OF THE TANAINA. *Yale University Publications in Anthropology, Number 16.*

By Cornelius Osgood. Yale University Press, New Haven; Oxford University Press, London. $3.00. 9¼ x 7; 229 + 14 plates; 1937 (paper).

The Tananina are a group of Athapaskan-speaking people living on the south coast of Alaska. Their numbers are few, having dwindled from an estimated population of 3000 in 1805 to an estimated 640 in 1932. In this monograph the author has endeavored to reconstruct and "present descriptively the manifest culture of the Tanaina as it would have appeared just previous to historic contact, or in the third quarter of the eighteenth century". This undertaking was rendered particularly difficult because the aboriginal culture has almost entirely disappeared and even the oldest members of the group recall but vaguely earlier tribal traditions and customs. In spite of many difficulties involved in the task, however, the author has accumulated much valuable data on food, dress, shelter, travel, manufactures and implements, war, arts and amusements, social organization, social customs, religion and mythology. Several plates and a bibliography are included.

PRELIMINARY REPORT ON THE EXPEDITION TO SAN AGUSTIN (COLOMBIA). *June–November 1936. Part I. Anthropological Series of the Boston College Graduate School, Vol. II, No. I.*

By Hermann von Walde-Waldegg. Boston College Press, Chestnut Hill, Mass. $1.00. 9¼ x 6⅝; 55 + 2 folding maps + 14 plates; 1937 (paper).

An account of the author's excavations at San Agustin, a site about 300 miles southwest of Bogota, near the southern border of Colombia, where important remains of a prehistoric Indian culture are found. Besides a large number of statues, the author found what he interprets as a calendar stone. Unfortunately, the remains which had been described by Preuss had suffered in the succeeding years from the depredations of treasure hunters. The plaster casts and photographs which Professor von Walde-Waldegg made will form a permanent record of his discoveries.

THE CACTUS EATERS.

By Julian A. Weston. H. F. and G. Witherby, London. 10s. 6d. 8⅝ x 5⅞; 240; 1937.

The cactus eaters are the inhabitants of a wasteland, the Goajira Desert, on the borders of Venezuela and Colombia. The object of the expedition was to take pictures, write a book afterwards, and, if possible, make money via both routes. The result is a very readable and entertaining account of life among the Goajira people. Excellent photographs have been included in large number.

AFRICA'S GOD. *V. Congo and Angola. Anthropological Series of the Boston College Graduate School, Vol. II, No. 2.*

By Joseph J. Williams, S. J. Boston College Press, Chestnut Hill, Mass. $1.00. 9¼ x 6¼; 137; 1937 (paper).

This book examines the evidence for the belief in a Supreme Being among the Bantu-speaking tribes of Congo and Angola. The author concludes that the belief is held by these tribes but that it is much less precise than in former times. A bibliography of 5 pages is given.

MEDICAL RELIEF ADMINISTRATION. *The Experience in Essex County, Ontario.*

Essex County Medical Economic Research, Windsor, Ontario. 9 x 6; 55; 1937 (paper).

This report describes policies and procedures of medical administrative techniques as carried out on a population on relief. The information given on methods of recording and on control formulas (averages, correlations, probable errors) can be found in numerous elementary statistical text-books.

BULLETIN DER SCHWEIZERISCHEN GESELLSCHAFT FÜR ANTHROPOLOGIE UND ETHNOLOGIE 1936/37. *13. Jahrgang. [Bulletin de la Société suisse d'Anthropologie et d'Ethnologie 1936–37. 13me année.]*

Schweizerische Gesellschaft für Anthropologie und Ethnologie, Sempersteig 3, Zürich 1. 9 x 6¼; 22; 1937.

THE COLORADO LABOR MARKET AND ITS RELATION TO UNEMPLOYMENT COMPENSATION. *The University of Colorado Studies, Volume 24, Numbers 3 and 4.*
By Edward R. Livernash. University of Colorado, Boulder. $2.00. 10 x 6¾; 72; 1937 (paper).

A HISTORICAL SUMMARY OF STATE SERVICES FOR CHILDREN IN OHIO. *U. S. Department of Labor, Children's Bureau, Bureau Publication No. 239.*
Government Printing Office, Washington. 10 cents. 9¼ x 6; v + 38; 1937 (paper).

ZOÖLOGY

OYSTER BIOLOGY AND OYSTER-CULTURE *being the Buckland Lectures for 1935.*
By J. H. Orton. Edward Arnold and Co., London. 5s. 7 x 4⅝; 211; 1937.

This work is extremely timely, for unless the oyster industry can be put on a scientific basis it is likely to suffer and even disappear. The indiscriminate over-fishing of oyster beds has dangerously reduced their production, and the unsupervised introduction of foreign spat has too often been accompanied by that of exotic pests, such as the New England Oyster Drill, *Urosalpinx cinerea*, into Europe and California.

The author discusses in detail the anatomy, growth, alimentation, and reproduction of three species of oyster—one from England, one from Portugal, and one from the eastern United States, but there is much incidental material included concerning oysters from the Pacific coast of America, Asia, and Australia, as well as from France. It was formerly believed that the so called "white-sick" and "black-sick" stages of the European Oyster were indications of sexual differences, but according to this work it is now know that the color is due to the age of the larvae remaining within the parental shell. In the American oyster the eggs are spawned directly into water and there are no color stages. Further, in the European oyster the change of sex occurs immediately after spawning, so that an oyster full of embryos is more likely to be male than not.

The work is not complete, however, for there is no mention of *Pinnotheres*, a crab the female of which lives symbiotically within the mantle cavity of the oyster, the existence of which was known as far back as Pliny's day. Also, the author recommends the transplanting of oysters to relatively fresh water to fatten them for the market, on the ground that the organisms on which it feeds are more plentiful there, but he does not tell us how much of this apparent fattening is merely bloating due to the difference in osmotic pressure, nor does he discuss the deterioration of flavor in fresh water oysters.

There is no index, but there is an interesting glossary of terms used by oyster fishermen.

THE DISTRIBUTION, BREEDING AND FEEDING OF SOME IMPORTANT PLANKTON ORGANISMS OF THE SOUTH-WEST NORTH SEA IN 1934. *Part I. Calanus finmarchicus (Gunn), Sagitta setosa (J. Müller), and Sagitta elegans (Verrill). Fishery Investigations, Series II, Vol. XV. No. 3.*
By R. S. Wimpenny. His Majesty's Stationery Office, London. 3s. net. 10¾ x 7¼; 56 + 2 plates; 1937 (paper).

EXPERIMENTS IN THE BREEDING OF OYSTERS (*Ostrea edulis*) IN TANKS, WITH SPECIAL REFERENCE TO THE FOOD OF THE LARVA AND SPAT. *Fishery Investigations, Series II, Vol. XV, No. 4.*
By H. A. Cole. His Majesty's Stationery Office, London. 2s. net. 10¾ x 7¼; 28 + 4 plates; 1937 (paper).

THE NATION'S SEA-FISH SUPPLY *being the Buckland Lectures for 1936.*
By E. Ford. Edward Arnold and Co., London. 3s. 6d. 7 x 4¾; 112; 1937.

The first article deals with the distribution, breeding, and feeding of *Calanus finmarchicus*, *Sagitta setosa*, and *Sagitta elegans*. Successful broods are dependent on diatom growth and further evidence is offered that the organisms breed in diatom patches which act as nursery grounds.

After several years of unsuccessful experiments, Cole feels that spatfalls of

commercial value can now be obtained from tank-bred oysters. Controlled organic enrichment of the water enables a selective growth of flagellates which practically assures a growth and settlement of oyster larvae. While the spat are capable of digesting the cellulose or hemi-cellulose cell wall of the flagellates, the larvae are unable to do so and must therefore restrict their diet to naked flagellates. A more detailed report is in preparation.

Mr. Ford makes a plea for the proper conservation of the nation's sea resources. He reveals the tremendous wastage that is prevalent and argues that the present intensity of fishing off European waters (applicable anywhere else) which gives little thought to the future will eventually deplete the stock. The Sea-Fishing Industry Act which gives the government power to control unrestricted freedom of fishing was a step forward. "... we stand at a great epoch in our fishing history; we have broken from the past in a tremendous way and now face the future with altered standards of fishing practice, in which state prescription takes the place of personal liberty."

We are always glad to learn of some progress that has been made toward the conservation of the world's natural resources. Somehow fish of the open sea have always been considered immune from extirpation because their aquatic environment makes it difficult to perceive any reduction in numbers, and also since the seas are so vast it does not seem possible that they could be exhausted. Regulating the size of the mesh of the nets is suggested by Ford as the remedy and in the second lecture he discusses this solution. The third lecture is based on the theme: "An ideal sea-fishery is one which is as much concerned about what it leaves behind in the sea, as about what it takes out of it."

Birds Collected by the Childs Frick Expedition to Ethiopia and Kenya Colony. *Part 2. Passeres. Smithsonian Institution, United States National Museum, Bulletin 153.*
By Herbert Friedmann. Government Printing Office, Washington. 70 cents. $9\frac{1}{4}$ x $6\frac{1}{4}$; xii + 506 + 14 plates; 1937 (paper).

This bulletin is the second publication on the ornithological collections made by the Childs Frick Expedition in Ethiopia and Kenya Colony, and is devoted entirely to the study of the Passerines. The book is based on the collections of birds, nests and eggs, and the observational data made by the late Dr. Edgar A. Mearns. The fact that Dr. Mearns was with the expedition in Africa only 10 months, and that during that time he collected 5200 birds besides a number of nests and eggs, and filled several notebooks with field notes, reveals, in a dramatic fashion, his personal persistence, enthusiasm, and industry.

The work gives a general account of the effect of geographic and climatic factors on bird behavior, and makes some attempt to describe the evolution of birds in Africa. A record of the number of specimens, the sex, the locality and the date of collection, together with the descriptions of color, song, general behavior and distribution is presented. Where natural history data are available, a description of the nesting site, nest, number, size and color of the eggs is also given. The text contains many tables incident to the variation of a species in different localities, and many maps showing the distribution of the different species and sub-species. There are 14 photographic plates showing different birds, nesting sites, and habitats. The table of contents and the index complete the monograph.

A First Guide to South African Birds. *By E. Leonard Gill. Maskew Miller, Ltd., Cape Town.* 8s. 6d. $7\frac{3}{4}$ x $5\frac{1}{4}$; xv + 223; 1936.

In this *First Guide to South African Birds*, Dr. Gill has met, in some degree, the demand for an introductory work on the country's birds. The study is not to be considered the last word on South African birds, either in the number of birds described, or in the detail with which they are described. The book is intended not for specialists in the field of ornithology,

but for beginners and tourists who may be interested in the South African birds.

The author has followed the general plan of giving for each species the relative size and color of the male and female, the general behavior, the song, the habitat and nesting site, together with the number, size and appearance of the eggs. For the migratory birds, there is also an account of the summer and winter residences, as well as the time of migration. For the naming of the different species, the author has drawn from the work of Mr. W. L. Sclater.

In addition to 20 full page colored plates picturing over 400 species, the book contains a short table of contents, an index of English and Afrikaans names, and an index of scientific (generic) names.

LIFE HISTORIES OF NORTH AMERICAN BIRDS OF PREY. *Order Falconiformes (Part 1). United States National Museum Bulletin 167.*

By Arthur C. Bent. Smithsonian Institution, Washington. 70 cents $9\frac{3}{4} \times 6$; 407 + 102 pates; 1937 (paper).

This bulletin is the tenth of a series devoted to the life histories of North American birds. The work is developed around the same general plan as has been followed in the previous numbers; namely, giving for each species a general account of the habits, nesting and eggs, followed by a description of the young, the plumages, food, general behavior, range, migration and casual records.

In the general accounts of each species, the author makes use not only of his own broad range of observations from field experience, but also all the important published material, as well as the notes of various amateur observers who have contributed their findings. The bulletin, therefore, represents the extent of present knowledge of our birds of prey (exclusive of the falcons).

The 102 plates present an enormous photographic record of the habits, nests, nesting sites, and appearances of the majority of the species described. The brief table of contents and the extensive bibliography are additional features of this excellent piece of work. We anxiously await the publication of Part 2, which will deal with the Falconidae.

A CATALOGUE OF THE AFRICAN HESPERIIDAE *Indicating the Classification and Nomenclature Adopted in the British Museum.*

By W. H. Evans. British Museum (Natural History), London. 20s. $9 \times 5\frac{3}{4}$; xii + 212 + 30; 1937.

The results of many year's untiring work of collecting and identifying the African Hesperiidae are brought together by General Evans in this catalogue. The author's knowledge of the Indian and Oriental Lepidoptera has been responsible for the detail and accuracy manifest in the preparation of this study.

The work begins with a key to genera which is arranged as explained in the introduction to that section. Then follows a brief diagnosis of each genus, describing points of difference from allied genera, a key to the species of that genus, a synonymic list of species with particulars and descriptions of sub-species and forms, ending with a list of material in the British Museum.

In the 7 colored plates are given figures of all species described for the first time, and of which figures have not hitherto been published. In the 30 plates of uncolored drawings are shown the male genitalia for each species in the Museum. There is an appendix of species described from Africa which have been found to occur elsewhere, or remain undetermined, a bibliography of about 250 references, and an index.

SNAKES ALIVE *and How They Live. Illustrated with Photographs and Including an Illustrated Key for the Identification of the Snakes of the United States.*

By Clifford H. Pope. The Viking Press, New York. \$2.50. $8\frac{1}{2} \times 5\frac{1}{2}$; xii + 238; 1937.

Beginning with his own story of his interest in snakes, his job in the Reptile House of the Bronx Zoological Park, and his adventures as a snake collector in China, the author goes on to a more de-

tailed discussion of the various kinds of snakes, their usefulness, their reproductive habits, their possible size and age, and the popular beliefs that have sprung up about them. On the whole it is a well written account and makes interesting reading. However, here and there it seems as if the author suddenly becomes aware of a moron reader and adds a sentence or two of irritatingly childish explanation. The volume is illustrated by splendid photographs and occasional drawings. There is an appendix containing a simplified key for the identification of the genera and species found in the United States, and a detailed index.

STUDIES IN THE LIFE HISTORY OF THE SONG SPARROW. I. *Transactions of the Linnaean Society of New York, Volume IV.*

By Margaret M. Nice. Linnaean Society of New York, c/o American Museum of Natural History, New York. $1.50. 9 x 6⅛; vi + 247; 1937.

To find a study in natural history that shows more patience and keen observation in the field than this, and more accuracy and selectivity in organization of material, would indeed be a difficult task. Over a period of eight years the author has made this intensive study of the habits and social behavior of the Song Sparrow, in an attempt to reveal some of the factors influencing its population, not only from season to season of the same year, but from year to year.

Each of the 20 chapters of the study is briefly summarized, and the salient points of the entire study are brought together in a general summary. The bibliography of more than 200 references shows something of the author's thorough acquaintance with the subject. The table of contents, appendices I to V, and the indices (1) of subjects, and (2) of species, are features incident to the completeness of the monograph.

POST-MORTEM EXAMINATIONS OF WILD BIRDS AND MAMMALS. *U. S. Department of Agriculture, Miscellaneous Publication No. 270.*

By J. E. Shillinger. Government Printing Office, Washington. 5 cents. 9¼ x 6; 16; 1937 (paper).

As field workers engaged in activities involving wild life frequently come across sick and dead animals of various species, this publication has been written to assist them in noting abnormal conditions and in helping to eradicate them by reporting the facts observed to wild life disease-control specialists.

The author discusses the various causes of death and symptoms of diseases. With the aid of photographs he tells how to perform autopsies on mammals and birds, going into the external factors involved, the dissection, the condition of the internal organs, and the examination of the head. A separate paragraph is devoted to each of the six most common causes of wild life losses; space is given to suggestions for preserving specimens for study; directions for shipping them to the laboratory; and hints on safeguarding investigators against infection.

TERMITE CITY.

By Alfred E. Emerson and Eleanor Fish. Foreword by William Beebe. Illustrations by Keith Ward. Rand McNally and Co., New York. $1.50. 8½ x 6½; 127; 1937.

This is the story of the highly socialized life of a termite colony and the routine of its various inhabitants. There is the bloated queen and her mate who spend their long lives of from twenty to fifty years in a single cell of the termitarium, producing tiny workers and slightly larger soldiers to build and to protect their nest, and other queens and kings who fly away to start new colonies.

It is a story that should captivate readers of all ages, beautifully and simply written, yet without a trace of the artificial simplicity so often found in children's books. As Beebe so aptly remarks in the foreword, "It is wholly free from such absurdities as 'Tommy Termite' and 'Nellie Nymph'."

A glossary supplements the text describing in more detail, but with the same simple clarity, many of the topics mentioned only briefly in the story. There is also a complete index.

FLIGHT SPEED OF BIRDS. *U. S. Department of Agriculture, Circular No. 428.*
By May Thacher Cooke. Government Printing Office, Washington. 5 cents. $9\frac{1}{4}$ x 6; 14; 1937 (paper).

As the possible flight speed of birds, usually computed on estimations alone, has always been greatly exaggerated, the author sets about to give some actual facts on the subject. In this paper she discusses individual speed variation among the same species, and the effect of size and shape of wings, the weight of the bird, and the influence of the wind on speed. In conclusion she gives a table of speeds showing the miles per hour flown by over a hundred different species as timed by airplane, automobile, ship, etc. A few running speeds are also included. The source of all the material, excellently summarized in the table, is given in a complete bibliography.

NEMATODES PARASITIC IN ANIMALS.
By Geoffrey Lapage. Methuen and Co., London. 4s. 6d. net. $6\frac{3}{4}$ x $4\frac{1}{4}$; x + 172; 1937.

THE NEMATODE, ORNITHOSTRONGYLUS QUADRIRADIATUS, A PARASITE OF THE DOMESTICATED PIGEON. *U. S. Department of Agriculture. Technical Bulletin No. 569.*
By Eugenia Cuvillier. Government Printing Office, Washington. 10 cents. $9\frac{1}{4}$ x $5\frac{7}{8}$; 36; 1937 (paper).

The book by Lapage is primarily a topical summary of the works of many workers, with conclusions drawn by the author. Nematode physiology is stressed, along with the need for further study in this field by experimental biologists so that better control measures may be determined.

In the Cuvillier bulletin is presented the study of a serious infection of pigeon flocks. Control birds were used to determine the relations between host and parasite, and the effect of external environment on eggs and larvae, in order to show methods of control valuable to pigeon raisers.

RAPPORT ATLANTIQUE 1934–1936 (*Travaux du Comité du Plateau Continental Atlantique*). *Rapports et Procès-Verbaux des Réunions, Volume CIV.*
Conseil Permanent International Pour l'Exploration de la Mer. Published with the aid of Ed. Le Danois and Rafaël De Buen. Andre. Fred. Host and Fils, Copenhagen. Kr. 2.00. $10\frac{1}{2}$ x $8\frac{1}{4}$; 37; 1937 (paper).

The Atlantic Slope Committee was created for the purpose of establishing a liaison between the *Conseil International pour l'Exploration de la Mer* and the *Conseil International de Recherches des Pêcheries de l'Amerique du Nord.* The object of the Committee is to conduct research on the problems common to both the American and the European coasts of the Atlantic Ocean. This report describes in some detail the studies that have been made in 1935 and 1936 and that have been presented at the annual meeting of the International Council.

DIE LACHSARTIGEN (*Salmonidae*). *I. Teil. Handbuch der Binnenfischerei Mitteleuropas, Band III, Lieferung 5.*
By E. Neresheimer. E. Schweizerbart'sche Verlagsbuchhandlung, Stuttgart. In Germany: 25 marks; Outside of Germany: 18.75 marks. $10\frac{1}{2}$ x $7\frac{1}{2}$; vi + 152; 1937 (paper).

This treatise on the various species of salmon found in Central Europe, deals with the anatomy, development, mode of life, mating, breeding grounds, and interrelationships between species. It is profusely and well illustrated and though primarily intended for the research worker or fishery official, can be used to advantage by the layman. There is a bibliography and an index.

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society. Volume XXII, Part 2, Numbers 7–13. Containing following articles: The Templeton Crocker Expedition VI. Oxystomatous and Dromiaceous Crabs from the Gulf of California and the West Coast of Lower California,* by Jocelyn Crane; *VII. Caridean Decapod Crustacea from the Gulf of*

California and the West Coast of Lower California, by Fenner A. Chace, Jr.; *VIII. Polychaetous Annelids from the West Coast of Lower California, the Gulf of California and Clarion Island*, by Aaron L. Treadwell; *IX. Holothurians from the Gulf of California, the West Coast of Lower California and Clarion Island*, by Elisabeth Deichmann; *Notes on the Cestodes of North American Sparrows*, by H. W. Stunkard and John J. Milford; *Further Studies on the Susceptibility and Acquired Immunity of Marine Fishes to Epibdella melleni, a Monogenetic Trematode*, by Ross F. Nigrelli; *Further Notes on Certain Birds of Paradise*, by Lee S. Crandall.

New York Zoological Society, Zoological Park, New York. $1.40. 10½ x 7; 97–195 + 5 plates; 1937 (paper).

A LIST OF THE BEETLES OF SAN DIEGO COUNTY, CALIFORNIA. *Occasional Papers, San Diego Society of Natural History, Number 2.*

By Ian Moore. Society of Natural History, San Diego, Calif. 8¼ x 5¼; 109; 1937 (paper).

A FURTHER REPORT ON BIRDS FROM SONORA, MEXICO, WITH DESCRIPTIONS OF TWO NEW RACES. *Transactions of the San Diego Society of Natural History, Volume 8, No. 23.*

By A. J. van Rossem and The Marquess Hachisuka. Society of Natural History, San Diego, Calif. 10¼ x 7; 16, 1937 (paper).

THELAZIA CALIFORNIENSIS, A NEMATODE EYE WORM OF DOG AND MAN, WITH A REVIEW OF THE THELAZIAS OF DOMESTIC ANIMALS. *University of California Publications in Zoology, Volume 41, Number 17.*

By Charles A. Kofoid, Owen L. Williams, and N. C. Veale. University of California Press, Berkeley. 25 cents. 10¼ x 6¾; 9; 1937 (paper).

THE ACTION OF SPECIFIC IMMUNE SERUM ON INFECTIONS OF TRYPANOSOMA HIPPICUM DARLING IN THE RAT. *University of California Publications in Zoology, Volume 41, Number 18.*

By Horace F. Sharrocks. University of California Press, Berkeley. 25 cents. 10¼ x 6¾; 14; 1937 (paper).

THE NEUROMOTOR SYSTEM OF NYCTOTHERUS HYLAE. *University of California Publications in Zoology, Volume 41, Number 19.*

By Lauren E. Rosenberg. University of California Press, Berkeley. 35 cents. 10¼ x 6¾; 27; 1937 (paper).

GENE AND CHARACTER. IV-VIII. *University of California Publication in Zoology, Volume 41, Numbers 20–24.* Containing the following articles: *IV: Further Data on the Development of Wing Mutants in Drosophila* and *V: Further Data on the vg Dominigenes in Drosophila melanogaster*, by Richard Goldschmidt; *VI: Dominigenes and vg Allelomorphs*, by Richard Goldschmidt and Elizabeth Höner; *VII: The "Nonhereditary" kn Effect in Drosophila* and *VIII: A Selection Experiment with Dominigenes*, by Richard Goldschmidt.

University of California Press, Berkeley. 75 cents. 10¼ x 6¾; 65; 1937 (paper).

BOTANY

THE GARDEN OF GOURDS.

By L. H. Bailey. Macmillan Company, New York. $2.50. 9¼ x 6; 134; 1937.

Gourds are so simple to cultivate that this book is not a horticultural guide but rather a series of descriptions of the many different groups. The gourd family is a large one including all the melons, cucumbers, pumpkins and squashes. The gourd of North America is defined as "a hard-shelled durable fruit grown for ornament, utensils and general interest." Their present popularity is indicated by the formation of the International Gourd Society in California. The organization publishes a semi-annual bulletin.

The author's enthusiasm over the diversities and beauties of the gourds is highly infectious. The reader immediately wants to try some in his own garden. Dr. Bailey says that he "has grown gourds in each year since his youth." He has, therefore, collected an enormous amount of information on the subject. His pen and ink drawings add greatly to the interest and artistic value of the book.

The historical ancestry of the gourd family is traced back to pre-Linnaean time. This early period is graphically shown in a set of reproduced illustrations some of which go back to Lobel in the sixteenth century.

There is a colored frontispiece and a comprehensive index.

STUDIES ON THE DEVELOPMENT OF CONIFERS IN RAW HUMUS. III. *The Growth of Scots Pine (Pinus silvestris L.) Seedlings in Pot Cultures of Different Soils Under Varied Radiation Intensities. Meedelanden Från Statens Skogsförsöksanstate, Häfte 29, Nr. 7*

By P. R. Gaast. Reprinted for the Harvard Forest, Petersham, Mass. Free. $6\frac{7}{8} \times 9\frac{3}{4}$; 587–682; 1937 (paper).

This represents a physiological study of the growth of pine seedlings. Scots pine and white pine (*Pinus strobus* L.) were grown under controlled conditions. The author believes that the limits of experimentation by varying a single factor have practically been reached. His work, therefore, involves the simultaneous variation of two factors: mineral nutrition and radiation. The paper contains not only detailed descriptions of the experiments performed and their results, but also statistical analysis of the results. In order to determine the effects upon the seedlings of varying two factors, other factors such as seed-weight must be carefully considered. By applying mathematical formulae the author has determined relationships, some logarithmic, some proportional, etc., which enable him to separate these various effects.

Tables, graphs, a list of references, and a summary in Swedish (some of the experiments were done at the Swedish Institute for Experimental Forestry) are included.

WÖRTERBUCH DER DEUTSCHEN PFLANZENNAMEN. *Lieferung 1, Abelia—Agrimonia.*

By Heinrich Marzell. Unter mitwirkung von Wilhelm Wissmann. S. Hirzel, Leipzig. RM. 5. $11\frac{1}{4} \times 8\frac{1}{4}$; x + 144; 1937 (paper).

This ambitious work is an attempt to list approximately eighty thousand vernacular German names of plants under the corresponding generic terms alphabetically arranged. It is being issued serially and the first installment runs from Abelia to Agrimonia.

Although German scientists have carried their researches to the ends of the earth they have contributed little to this work, for the scientist uses the more exact terminology of his science and can get along very well without vernacular names. The only plants having German vernacular names are those growing in countries where this language is spoken, such as in central Europe, those growing in regions settled by German immigrants, such as Lancaster County, Pennsylvania, or those which have a pharmaceutical or commercial value, so that their products are imported into Germany. The geographic ranges of the plants covered are therefore very variable and inexact: this however, is not a disadvantage when the purpose for which the work was undertaken is considered. It is a dictionary, and the metes and bounds of dictionary definitions must be essentially linguistic rather than geographic. The illustrations are well drawn and the type is good. Altogether, the work is likely to prove useful.

PHYTOHORMONES.

By F. W. Went and Kenneth V. Thimann. The Macmillan Co., New York. $4.00. $8\frac{3}{8} \times 5\frac{1}{2}$; xi + 294; 1937.

As our knowledge in this field has progressed the terms "growth substance" and "growth hormone" have been replaced by the more specific term "auxins," applying to a group of substances affecting cell elongation. In this volume the authors deal only with the hormones of the higher plants. They attempt to show how "correlation proper, organ formation, tropisms and normal growth have . . . . been unified into a complete picture of hormone activity as we now know it."

The book includes introductory background, detailed descriptions of experimental technique, results of experiments, and new theories. Some of this material has not previously been published. The chief fault of which the authors can be accused is over-enthusiasm. As often happens in a fairly new and rapidly developing field, there is a tendency to attribute practically the entire development of the plant to hormone activity.

Time usually suffices to produce the proper perspective. But of course there is always the possibility that in this case the proper perspective has already been achieved. We can but wait and see. In the meantime, however, the authors have given us an excellent summary of the present situation.

STUDIES ON WHEAT GROWN UNDER CONSTANT CONDITIONS: *a Monograph on Growth.*
By H. L. van de Sande-Bakhuyzen. With contributions by Elizabeth P. Griffing and Carl L. Alsberg. Food Research Institute, Stanford University, Cal. $4.00. 8½ x 5¾; xvi + 400; 1937.

The object of this study was to determine the "standard wheat plant" or the inherent, intrinsic rate of growth of the wheat plant. With all environmental conditions controlled leaf areas were measured, growth curves plotted, dry weight, carbon and nitrogen content determined, etc. The hope was that the results of the study would facilitate more accurate forecasting of crop yields from the weather, "the weather" in this case being considered as a single variable. The observations and analyses are extremely detailed and all sorts of relationships and correlations have been determined. The Directors of the Food Research Institute, the instigators of this investigation, do not consider this piece of work as a complete solution to the problem; rather it is to be considered as a beginning for future work.

The text is illustrated with graphs and tables, and an index and detailed bibliography are included.

HARDY CALIFORNIANS.
By Lester Rowntree. The Macmillan Company, New York. $3.50. 8½ x 5½; xiv + 255; 1936.

This book introduces its readers to many of the hardy but less familiar native wild plants of California. The author is a plant enthusiast who has literally dug into all niches and corners of the state to observe and study the several hundred plant species so colorfully described here. For the garden maker who would like to learn the secrets attending successful culture of wild plants in a new environment, careful notes have been made on exposure, type of soil and other requirements. The author has found that many California plants generally considered tender are, in reality, quite hardy if planted in the proper environment. The book is illustrated with about seventy photographs taken by the author, of close-ups of plants growing in their natural habitats. The volume is indexed.

BACTERIOLOGY. *A Text-Book of Microörganisms. Third Edition.*
By Fred W. Tanner. John Wiley and Sons, New York; Chapman and Hall, London. $3.50. 9 x 5⅞; xiii + 510; 1937.

The first edition of this text received notice in Vol. 4, No. 3 of the Quarterly. The present edition is, in essence, an exact duplicate of the earlier one with few minor differences. The author has made a few additions, alterations, and omissions in his arrangement and discussion of material, and has included about a dozen new illustrations in the text. He has also brought the bacteriological literature up to date.

The text is developed around the fundamentals of bacteriology, and so is intended more for the beginner than for the advanced or special student in microbiology. It contains a table of contents, an appendix of bacteriological literature, a glossary, a topical outline for lectures in microbiology, and a lengthy index.

LA VIE DE LA CELLULE VEGETALE. *L'Enveloppe de la Matière Vivante.*
By Raoul Combes. Armand Colin, Paris. 13 francs. 6¾ x 4½; 216; 1937 (paper).

This is the third volume of the author's intensive study of the plant cell; the first two being devoted to *La matière vivante*, and *Les enclaves de la matière vivante*. In the present volume, M. Combes discusses (1) the morphological, physical and chemical structure, together with the general physiology, of the cell membrane; and (2)

the morphological and chemical structure, together with the general physiology, of the bodies that collect either in the cell membrane or in the intercellular spaces.

The work is based largely on the author's own experimental studies, but it contains some 250 citations to literature relative to the subject. The book contains a number of interesting drawings and diagrams, a short table of contents and a bibliographic index.

Floral Morphology. *A New Outlook with special reference to the Interpretation of the Gynaceum. Volume I.*

By E. R. Saunders. W. Heffer and Sons, Cambridge. 3s. 6d. $7\frac{1}{4}$ x $4\frac{3}{4}$; viii + 132; 1937.

Unlike systematic botany which treats the subject of floral morphology almost entirely on a basis of the external characteristics of the plant, this book considers the external features in relation to the vascular system of the plant. This new approach has led to the solution of many morphological problems and, in other cases, to the simplification of previously accepted explanations. The first part of the book consists of an exposition of the general principles which are regarded as underlying floral arrangement. The remainder, and larger portion, discusses thirty-nine Families considered from the new point of view. The author hopes to publish a second volume which will deal similarly with other families.

General and Economic Botany.

By Ernest E. Stanford. D. Appleton-Century Co., New York. $4.00. $8\frac{3}{4}$ x $5\frac{7}{8}$; xxix + 675; 1937.

This book has been designed as a text for elementary college botany. It differs from a large number of other introductory texts in that it emphasizes the economic rather than the purely scientific aspect of plants. This being so, it is natural that the main approach should stress morphology rather than physiology since "the principal significances and utilities of plants derive more obviously from form and composition than from function." As for content, the usual first-course-in-botany material is covered, although perhaps more adequately from the point of view of general information than as a foundation for advanced work in the field. Interesting drawings and photographs and an index are included.

Plants Useful to Man. *Second Edition.*

By Wilfred W. Robbins and Francis Ramaley. P. Blakiston's Son and Co., Philadelphia. $3.50. $8\frac{1}{2}$ x $5\frac{5}{8}$; ix + 422; 1937.

The three principal improvements of this edition over the first (reviewed in Volume 9, page 113 of this Quarterly) are (1) additions to, or modifications in the accounts of alfalfa, flax, figs, potato and various ornamental plants; (2) a rearrangement of parts of the chapter on sources of cultivated plants in order to bring out more clearly the original native homes of plants and to emphasize the early centers of agriculture; and (3) replacement of some of the figures.

On the Place of Ontogeny in Floral Enquiry. *Publications of the Hartley Botanical Laboratories, No. 17.*

By John McL. Thompson. University Press of Liverpool, Liverpool. 3s. 6d. 12 x $8\frac{1}{2}$; 20; 1937 (paper).

This is a technical discussion based on an article by Dr. Kozo-Poljanski which in its turn was based chiefly on the work of Dr. H. H. Thomas and Professor Thompson. The author has written this paper to clarify "some of the conceptions to which special reference has been made" and for this reason has considered it appropriate to examine the basis of Dr. Kozo-Poljanski's objections.

Silva Fennica 39. *Metsänhoitajien Jatkokurssit. [Der Fortbildungskursus der Forstmeister 1935.]*

Society of Forestry in Suomi, Helsinki. $9\frac{3}{4}$ x $6\frac{1}{2}$; 310; 1937 (paper).

ACTA FORESTALIA FENNICA 44. *Publications of the Society of Forestry in Suomi.*
Helsinki. 9⅝ x 6½; 468; 1937 (paper).

ETUDES DE DEVÉLOPPEMENT FLORISTIQUE EN LAURENTIE. *Contributions du Laboratoire de Botanique de l'Université de Montréal No. 27.*
By Frère Cléonique-Joseph. Institut Botanique, Université de Montréal, Montréal. (Obtainable also from Henry G. Fiedler, New York and T. Oswald Weigel, Leipzig). $1.50. 9 x 6; 246; 1936 (paper).

MORPHOLOGY

STRUCTURE OF THE VERTEBRATES. *Revised Edition.*
By Malcolm E. Little. Farrar and Rinehart, New York. $3.00. 8¼ x 5½; x + 488; 1937.

Presented by a professor of education who is thoroughly versed in comparative morphology, the material of this college text is well organized for both teacher and student. It is capable of fairly close correlation with a laboratory course. The book omits much of the detail in histology, embryological development, and anatomy "in order to help the student gain a better understanding of fundamental relationships" in the evolution of the structure of man. Accordingly. each system considered leads in this direction, and the final chapters are devoted to evolution. There are many good drawings, a complete index, and a glossary which includes Latin or Greek derivations.

VERGLEICHEND-ANATOMISCHE, EXPERIMENTELLE UND EMBRYOLOGISCHE UNTERSUCHUNGEN ÜBER DAS NERVENSYSTEM UND DIE SINNESORGANE DER RHYNCHOTEN. *Zoologica, Heft 93, Lieferung 1 and 2.*
By Otto Pflugfelder. E. Schweizerbart'sche Verlagsbuchhandlung, Stuttgart. In Germany: 74 marks; Outside of Germany: 55.50 marks. 12½ x 9¾; vi + 102 + 25 plates; 1936 (paper).

This is a thorough piece of research on the nervous system of the Rhynchota. After a description of the material used, the author describes the external morphology of the Heteroptera and Hymenoptera. There follows a long histological discussion of the brain of various species of this order of insects. The last section deals with experimental work involving removal or destruction of sense organs; the effect of this on the brain was unnoticeable. There is a bibliography and index, also many excellent plates and drawings.

LABORATORY STUDIES IN COMPARATIVE ANATOMY.
By W. C. Senning. McGraw-Hill Book Company, New York. $1.75. 9 x 6; ix + 188; 1937.

An excellent laboratory manual for the teaching of elementary comparative anatomy. Very little is done with type forms, the main emphasis being on a study by systems of organs. The shark, *Necturus*, and the cat are the animals used, student dissections being supplemented by frequent use of demonstration specimens.

GRUNDRISS DER ENTWICKLUNG DES MENSCHEN. *Zweite, neu bearbeitete Auflage.*
By A. Fischel. Julius Springer, Berlin. RM. 12.60. 9¾ x 6½; v + 143; 1937.

The first edition of this work was reviewed in Volume 7, Number 2 of the Q.R.B. The second edition follows the first in outline, with here and there reference to more recent work in human embryology. This work still suffers from the lack of a bibliography.

PHYSIOLOGY AND PATHOLOGY

MECANISMO PROBABLE DE LA CANCERIZACIÓN. *(Ensayo Patogénico). Two Volumes.*
By Americo Garibaldi. Facultad de Ciencias Médicas, Universidad Mayor de San Marcos, Lima. 10 x 7; Vol. 1, xxxii + 281; Vol. 2, 327; 1936 (paper).

The author presents here a theory to end all theories regarding the mechanism of cancer growth. His theory results from the attempted integration of all the basic

observations which have led to the numerous hypotheses relative to the mode of development of cancer. He reviews critically the assumptions underlying a number of them, over 30 in fact, and concludes that all are in some degree inadequate because they do not interpret the facts in terms of cellular physio-pathology. Briefly put, his own theory is based on the assumption that a cell that will develop cancer is characterized by unstable metabolism and altered surface tension. This would explain the hypercholesterinemia and hyperglycemia so often found and it would also explain the increased size of the nucleus relative to the cytoplasm, the only real morphologic change to be observed in a cancerous growth. Further considerations on this point lead him to state that a cancerous cell is a cell with a tendency towards nuclear and cytoplasmatic hypertrophy, but which reacts to this tendency through the process of proliferation. To explain cancer formation he contends that it represents an activity analogous to the reaction of normal tissue to infection. With this in mind he proceeds to the final formulation of his theory: Cancer is a defense mechanism against the metabolic insufficiency and disturbance of surface tension of the cell.

The author's *tour de force* is admirable even though the excessive rationalization based on insecure grounds is not so convincing. He deserves to be read. The bibliography is extensive but the citations have not been given in complete form.

Some Quantitative Aspects of the Biological Action of X and γ Rays. *Medical Research Council, Special Report Series, No. 223.*

By C. M. Scott. His Majesty's Stationery Office, London. 1s. 6d. net. $9\frac{1}{8}$ x 6; 99; 1937 (paper).

It is assumed that radiation which passes directly and unchanged through a tissue has no biological effect, the changes, if any, which occur being produced by fluorescent absorption and by the scattering of radiation with consequent change in wave length. In order to measure the dose of radiation administered, the *r* unit, based upon the absorption of radiant energy, in air, is in common use today. Unfortunately, this does not take into account the great variation in absorption according to size, shape, substance, and depth of tissue being treated. Due to this difficulty of measurement of dose, even when an attempt at measurement is made at all, and to great variation in techniques used, many workers have obtained directly conflicting results. In spite of the difficulties, Scott has done an admirable piece of work in summerizing and comparing the major findings of workers in this field. In most instances, the therapeutic value of radiation is dependent upon its differential effect on normal and on abnormal or diseased tissue. It is therefore important to discover how various tissues differ in this respect and what artificial factors may be introduced to modify their sensitivity to radiation. Scott tells what evidence there is in regard to this, adds a considerable amount of experimental evidence of his own, and has done much to clarify the issues. As the brochure is short and clearly written, those in other fields of biology should find it interesting reading. The bibliography is up to date and quite extensive.

Les Épidémies et l'Histoire.

By Albert Colnat. Éditions Hippocrate, Paris. 30 francs. $9\frac{5}{8}$ x $6\frac{1}{8}$; 191; 1937 (paper).

This monograph introduces what might be called an epidemiological view of history. Man is and always has been a victim of disease, in the past as now. This has affected his mode of living and his activities. Consequently the sequence of past events which we call history has been affected to a greater or less degree by the presence or absence of disease. In this respect, the epidemic diseases whose actions are manifested simultaneously on a mass of people are of particular importance in determining certain changes in the course of events. The author recalls how greatly the Pest at Athens contributed to the downfall of that city during the Peloponnesian Wars. He notes that the series of epidemics known as the

Antonine Pest and the Justinian Pest probably contributed more to the disruption of the Roman Empire than did the barbarians. The influence of epidemics on the outcome of wars is discussed in relation to the Crusades, to the Hundred Years War, and down to the World War, in which typhus was a factor in blocking the conquest of Serbia by the Austro-Hungarians.

The idea outlined here is far from original but it has still to be developed in an adequate manner. The author's attempt is a step forward although he has not really succeeded in investigating with any degree of thoroughness the complex ramifications of the relation between disease epidemics and the course of human events.

ENDOCRINOLOGIE CLINIQUE, THÉRAPEUTIQUE ET EXPÉRIMENTALE.

By P. Sainton, H. Simonnet and L. Brouha. Masson et Cie, Paris. 130 francs. 10 x 6¾; viii + 834; 1937 (paper).

This treatise covers in an authoritative manner the present state of knowledge regarding the normal and pathologic structure and function of the endocrine glands. The first chapter describes in sufficient detail the development of this branch of medicine and the methods of research. This chapter is especially well written and contains important information, usually omitted in the majority of textbooks. Following this, there are chapters on the thyroid, the parathyroids, the adrenals, the hypophysis, the pineal, the gonads, the thymus, and the pancreas. For each of these glands are specified the particulars of anatomy, physiology, biochemistry, experimental work, clinical symptoms of hyper- or hypo-function, and therapy. In separate chapters are discussed the function of the endocrines in pregnancy and in the various forms of intersexuality. The authors also present a critical review of the studies which have demonstrated or presumed to demonstrate hormones in the digestive tract, salivary glands, and other organs of the body. Throughout this book one notes that the authors have evaluated with a great deal of objectivity the discoveries and observations reported, and the reader is always made aware of the relative validity of the conclusions reached. Only one major criticism can be made: there is no bibliography.

VISUAL PERCEPTION.

By M. D. Vernon. The University Press, Cambridge; The Macmillan Co., New York. $4.50. 8½ x 5½; xi + 247; 1937.

This treatise is a "halting and incomplete" explanation of (1) the stages in the development of perceptions; (2) the relation of these perceptions to affective individual states; (3) the structure of the perceptual field; and (4) the development of the perceptual content during childhood.

The volume is written in a very technical manner, and emphasizes, in minute detail, the experimental procedures and observations carried out. Although the author has drawn extensively from Gestalt psychology, she is critical of the use of the theory as a general explanation for perceptions. The author's purpose has been to "dig the ground, clear away the debris, and perhaps prepare the foundation" for the structural explanation of perceptions in terms of psychology and physiology. The book is a careful achievement of this purpose.

The volume contains a bibliography of nearly 400 references, a detailed table of contents, an appendix describing and illustrating the tachistoscope, an index of authors and an index of topics.

CHILDBIRTH: *Yesterday and Today. The Story of Childbirth Through the Ages, to the Present.*

By A. J. Rongy. Emerson Books, Inc., New York. $2.00. 5⅛ x 7⅝; 192; 1937.

SAFELY THROUGH CHILDBIRTH. *A Guide Book for the Expectant Mother.*

By A. J. Rongy. Emerson Books, Inc., New York. $2.00. 5⅛ x 7⅝; 192; 1937.

These two volumes, each complete in itself, together form a connected story of childbirth from ancient times to the present day. From such a comparative study

one gains an appreciation of the advantages held by the prospective mother of today over her sister of past ages when in place of the scientific knowledge of the present, childbirth was attended by ignorance and superstitions.

The second of these books contains a short but up-to-date account of the development of the embryo and foetus with accompanying changes in the mother's condition, descriptions of the processes of labor and normal and operative births. There are chapters on pre-natal and post-natal care and spontaneous abortion. The book is sensible and practical and a good one to be placed in the hands of prospective mothers. Each volume has an index and the second is equipped with a glossary, but bibliographies are lacking.

Some Fundamental Aspects of the Cancer Problem. *Symposium Sponsored by the Section on Medical Sciences of the American Association for the Advancement of Science. Atlantic City, New Jersey, December 29, 1936—January 1, 1937.*

Edited by Henry Baldwin Ward. The Science Press, New York. \$2.50 (cloth); \$2.00 (paper). $10\frac{1}{4} \times 7\frac{1}{8}$; 248; 1937.

The purpose of this symposium was to give a survey of the latest work done by leading American investigators on the many sides of this problem. The thirty-one papers presented at Atlantic City are all included in this volume in their original form and with their original conclusions. Certain of these papers, such as *Genetics of Cancer and Its Localization* by Maud Slye, excited some very lively discussion at the meeting which unfortunately was not recorded at the time and so could not be included here. The symposium was divided into several sessions according to subject matter, seven papers being given on heredity and constitutional factors, nine papers on induction, stimulation and inhibition of tumorous growths, five papers on metabolism of cancerous tissue, and seven papers on the effect of radiation. Louis L. Dublin gave a *Statistical Analysis of Mortality from Cancer.* C. C. Little discussed the *Social Significance of Cancer*, and Walter Schiller reviewed *Changes and Modifications in the Conception of Carcinoma* in a final session for the general discussion of the cancer problem.

Textbook of General Physiology.

By T. Cunliffe Barnes. P. Blakiston's Son and Co., Philadelphia. \$4.50. 9×6; xxii + 554; 1937.

In the past we have found it necessary to criticize text books on this subject on the score of being dogmatic and of giving neither proof nor references for their statements. We are, therefore, glad to say that Dr. Barnes prefers to cite the evidence rather than to state "laws", and that he gives numerous references to every subject dealt with, his bibliography listing over 1,600 papers. Naturally, no student is going to look up all these papers, but this presentation will encourage him to go more deeply into any phase of the subject that especially catches his interest.

The distribution of space and the subject matter covered is on the whole quite usual. Naturally, more than usual attention is given to the properties of water, since this is a field in which the author has done much work. This is rationalized on the ground that water is the most universally vital compound for living organisms.

The text is well written and there are many illustrations and diagrams. The inclusion of a number of semi-comic cartoons is a unique feature of the book.

Aids to Physiology. *Second Edition.*

By Henry Dryerre. William Wood and Co., Baltimore. \$1.25. $6\frac{1}{4} \times 3\frac{3}{4}$; vii + 295; 1937.

This book is clearly written and well compiled for its purpose, but we are not so sure that, its purpose being fulfilled, the results would be desirable. It is a sketchy and abbreviated outline of the subject, with the emphasis strongly placed on definitions of a great number of technical terms which are printed in bold face type. Its "aid" to students, so far as we can see, would be mainly in cramming for exami-

nations and this is why we are a little dubious as to its value. In our experience, one of the greatest benefits of an outline of this kind is derived from the actual making of it by the student himself, as this forces him to give close attention to the subject and impresses it upon his memory. On the other hand, workers in other fields might well find it convenient as an extended dictionary to refresh their memory when they have to read a physiological paper.

TRAUMA AND DISEASE.

Edited by Leopold Brahdy and Samuel Kahn. Lea and Febiger, Philadelphia. \$7.50. $9\frac{1}{4} \times 5\frac{7}{8}$; 613; 1937.

This symposium by 24 eminent physicians summarizes and reviews critically the data on the rôle of trauma as an etiologic factor in the development and progression of disease. The purpose of the book is to furnish information of the sort needed in legal medicine and industrial hygiene. The more important disorders of all the organ systems are discussed and for each the task has been assigned to a well-known specialist. With but few exceptions, the subject has been treated in a thorough fashion, the general symptomology of a disease is summarized and then the evidence regarding the effects of trauma on it and the clinical characteristics of this effect, are presented clearly and concisely. Each chapter is supplemented by an adequate bibliography. Not only the physician but the student of vital statistics and of public health will find this a useful book to consult.

TAKE CARE OF YOURSELF. *A Practical Guide to Health and Beauty. Stressing the Proper Way to Use and the Prudent Way to Buy Home Remedies and Cosmetics.*

By Jerome W. Ephraim. Foreword by Logan Clendening. Simon and Schuster, New York. \$2.00. $8 \times 5\frac{1}{2}$; xvi + 287; 1937.

As we flounder in the bewilderment of torrential advertising in the field of home remedies, cure-alls, and personal beautifiers, we welcome a book of this type. Whether it be insomnia, bunions, or "hang-over" that we are suffering from, *Take Care of Yourself* has some relief to offer. Mr. Ephraim is neither a physician nor a chemist, but he has spent much of his life in determining the value of many of our home remedies and cosmetics, and in advising the public as to the proper way to buy and use them. Although the author himself is a manufacturer and seller of drugs, cosmetics and toilet articles, he very cautiously refrains both from advertising his own brands, and from attacking the wares of his competitors. As Logan Clendening says in his forward, "Mr. Ephraim's statements . . . . stand exactly between the extremes of the advertisers and the debunkers of cosmetics and drugs."

The table of contents and topic index add to the value of the book as a home reference.

L'ANAPHYLAXIE *Expérimentale et Humaine.*

By Pasteur Vallery-Radot, G. Mauric and Mme Holtzer (ex-Hugo). Masson et Cie, Paris. 36 francs. $9\frac{3}{4} \times 6\frac{1}{2}$; 130; 1937 (paper).

The first part, comprising about two-thirds of this book, is a report of experiments on the phenomena of allergy and desensitization. Most of the experiments were performed on rabbits, and the proteins used were those most commonly allergic to man via the respiratory tract, namely, squama from horses, cat hair, orchard grass, and wheat flour. The records include the period from injection of the sensitizing substance to the time of desensitization. The second part is a brief and rather sketchy treatment of anaphylactic shock in man, skin reactions to various proteins, and treatment. There is little critical discussion of the work of others on this subject, and the bibliography is confined to publications from the senior author's laboratory. There is no index.

THE ENDOCRINES IN OBSTETRICS AND GYNECOLOGY.

By Raphael Kurzrok. Williams & Wilkins Co., Baltimore. $7.50. 9 x 6; xvi + 488; 1937.

Although this book presents applications of recent discoveries of endocrine physiology to the problems of obstetrics and gynecology, in particular, it should also be of interest to biochemists, general practitioners, surgeons, and biologists. It combines research, by the author and others, on the problems of sex and reproduction with clinical observations made on a very large material at Columbia Medical Center and at the Bronx and Morrisania City Hospitals. Most of the material follows human physiology, illustrations from animal physiology being utilized only when similar observations have not been made on man. Besides a treatment of all the hormones and endocrines the author includes material on menstruation, ovulation, amenorrhea, and sterility. Bibliographies are incorporated in each chapter, and author and subject indices are provided.

THE EFFECT OF VOLUNTARY ACTIVITY ON THE KNEE-JERK. *Comparative Psychology Monographs, Volume 14, No. 4, Serial No. 70.*

By Sarah C. Dunlap. The Johns Hopkins Press, Baltimore. $1.25. 10 x 7; 62; 1937 (paper.)

The problem of the investigation reported upon this monograph was "to determine whether a leg response practiced to a knee-jerk stimulus or its tactual component can become involuntary, and whether the reaction time of such a response may approach with practice that of the knee-jerk; and further, what effect the practice may have on the knee-jerk time." Tentative conclusions to the problem are drawn on the basis of the data obtained from fourteen subjects in the experimental set-ups described. In the author's opinion it is obvious that the knee-jerk, in the normal individual, is no simple segmental "reflex", but an integrated response, of a complex nature.

LA PATHOGÉNIE DES OEDÈMES. *Confrontation des Théories a la Clinique.*

By Pierre Mauriac. Masson et Cie, Paris. 16 francs. $7\frac{3}{4}$ x $5\frac{5}{8}$; 87; 1937 (paper).

The authors develop the thesis that transudative and exudative oedemas are produced by certain physico-chemical conditions in the tissues and humors of the body. The most important of these are disorders of the blood and lymph supply, mineral equilibrium, and metabolism of proteins and lipides, variations of pH and modifications of the capillary walls. These conditions, in turn, may be the consequence of cardio-vascular, renal and nervous disease, endocrine equilibrium or alimentary insufficiency. There appears to be no relation between pathogenic factors and the clinical type of oedema ultimately manifested. A bibliography has been supplied.

UNE FORME CÉRÉBRALE DE LA CHOLESTÉRINOSE GÉNÉRALISÉE (*Type Particulier de Lipose à Cholestérine*).

By Ludo van Bogaert, Hans J. Scherer and Émile Epstein. Masson et Cie, Paris. 45 francs. $10\frac{3}{4}$ x $6\frac{3}{4}$; 183; 1937.

A detailed and thorough account of this particular form of cerebral cholesterinosis, which is rather rare, is set forth here for the first time. In the first chapter the authors give the definition, history and classification of known forms of lipidosis. The second chapter gives a detailed clinical description of the form under consideration and discusses the possible importance of inherited constitutional factors. The third chapter is devoted to its histopathological characteristics and the fourth and last chapter is a biochemical and histochemical study of the deposits. This malady is apparently due to a metabolic maladjustment and not to an infection.

LES IMMUNITÉS LOCALES.

By A. Besredka. Masson et Cie, Paris. 35 francs. 9 x $5\frac{3}{4}$; 224; 1937 (paper).

The author's first book on this subject, published a little over a decade ago and now out of print, revolutionized the views on the biology of immunity. The present

volume, continuing the same thesis, presents newer findings, both experimental and clinical, on infections and the specific powers of antiviruses. The material is arranged according to organs, special sections being devoted to immunities of the skin, lungs, pleura, peritoneum, and intestines. One chapter treats plants in a similar way. Bibliographies conclude the separate chapters, and a detailed table of contents serves as index.

AN OUTLINE OF GENERAL PHYSIOLOGY.

By L. V. Heilbrunn. W. B. Saunders Company, Philadelphia and London. \$5.00. 9 x 5¾; 603; 1937.

Dr. Heilbrunn prefaces this text with a quotation from Pavlov:

". . . we are not far from a complete understanding of life as an association of organs . . . the beginning, the basis of life is in the cell." With this idea in mind, he has devoted a very large portion of the book to considerations of cellular metabolism.

The descriptions and explanations are clearly written and easy to read. A great number of references are given in footnotes which are listed in an alphabetical author index in the back of the book. The subject index is also very thorough.

THE PATIENT AND THE WEATHER. *Volume IV, Part 2, Organic Disease. Hypo- and Hyperthyroidism, Diabetes, the Blood Dyscrasias, Tuberculosis.*

By William F. Petersen and Margaret E. Milliken. Edwards Bros., Ann Arbor, Mich. \$11.00. 10¾ x 8¼; xxviii + 729; 1937.

Dr. Petersen's latest lengthy volume is in every respect similar to his previous ones which have been reviewed in this journal. Many new case records are presented of the type indicated in the sub-title. We cannot see how they add much evidence in support of his theory and while the descriptive material is straightforward and clear, his analyses in terms of weather is sketchy and confusing to say the least. Since he is doing so much work on the subject it is a pity that he does not take a large random sample of cases and present in tabular form the degree to which their physiological variations fit in with the variations in the weather.

LEÇONS DE PHYSIOLOGIE MÉDICO-CHIRURGICALE. *(Deuxième Série).*

By Léon Binet. Masson et Cie, Paris. 36 francs. 10 x 6½; 137; 1937 (paper).

The subject matter of this book is confined to a few points of immediate practical interest to the physician and especially the surgeon. Most of these are interestingly presented as experimental studies discussed in much more detail than is usually found in a text book. While it would not suffice for a course in physiology it should be a useful supplement to a standard, broad text book for students who intend to become surgeons.

L'HORMONE FOLLICULAIRE EN PHYSIOLOGIE NORMALE ET PATHOLOGIQUE. *Étude Expérimentale Clinique et Thérapeutique.*

By Henri Simonnet. Masson et Cie, Paris. 100 francs. 9¾ x 6¾; xii + 532; 1937 paper).

The author's aim is to demonstrate in what manner it is possible to conceive the rôle played by folliculin, in the normal state, in metabolism, and in disorders of which this hormone may probably be the cause. It is based on a voluminous literature (the bibliography runs to 2500 titles) of research and clinical observations, including some of his own work on the subject. Of interest to workers on various phases of sex and reproduction.

DER BLUTDRUCK DES MENSCHEN.

By Eskil Kylin. Theodor Steinkopff, Dresden and Leipzig. RM. 24 (paper); RM. 26 (cloth); 25 per cent reduction outside of Germany. 9½ x 6½; xv + 322; 1937.

This work is not intended to serve as a textbook or manual for the student or practising physician but rather to give a complete survey of the state of our knowl-

edge in the whole field of arterial tension and its regulation. Of great value is the literature list (pp. 262–316) which contains practically everything that has been written of real importance on the subject. The book is provided with illustrations and an index.

Oxygénothérapie et Carbothérapie. *Bases Physiologiques. Applications Cliniques. Techniques.*

By L. Dautrebande. Masson et Cie, Paris. 35 francs. $9\frac{3}{4} \times 6\frac{3}{8}$; 300; 1937 (paper).

This book will be of interest to the physiologist, physician or surgeon rather than to the biologist. It treats the physiological bases of the requirement and transport of oxygen and carbon in the human body and their administration in the treatment of cardiac, circulatory, respiratory, and nervous diseases. The bibliography contains over 1200 titles. There is no index.

Maternal Care. *The Principles of Antepartum, Intrapartum, and Postpartum Care for the Practitioner of Obstetrics. Approved by The American Committee on Maternal Welfare, Inc.*

Edited by F. L. Adair. University of Chicago Press, Chicago. 25 cents (paper); $1.00 (bound). $7\frac{5}{8} \times 5\frac{1}{8}$; v + 93; 1937.

It would not be an exaggeration to state that every medical practitioner who takes care of maternity cases will find this booklet highly useful. Of special note are the detailed instructions for the delivery of women in their own homes. Information is given for forceps operations, version and extraction, and breech delivery. The material is very well organized, and the presentation exceptionally good.

Les Phénomènes Electrodynamiques dans le Sang et le Moyen de les Diriger.

By A.-L. Tchijevsky. Éditions Hippocrate, Paris. 10 francs. $10 \times 6\frac{1}{4}$; 47; 1936 (paper).

This paper is a summary of the principles which have guided the author's work during the last 15 years in an effort to correlate the electrodynamic phenomena of the blood in states of health and disease. Although this work originated in Moscow it resembles in certain respects the teachings of Dr. George Crile in this country.

Annali dell'Istituto "Carlo Forlanini". *Pubblicazione Mensile. Anno 1. Numero 1, 2, 3. 1937.*

Attilio Omodei-Zorini, Editor. Istituto Nazionale Fascista della Previdenza Sociale, Gennaio. Annual subscription: L. 50 (Italy and Colonies); L. 70 (outside of Italy); L. 30 (special price for physicians belonging to the I.N.F.P.S.). Single number: L. 5. $9\frac{3}{8} \times 6\frac{5}{8}$; Numero 1, 82; Numero 2, 95; Numero 3, 116; 1937 (paper).

I Medicinali e il Metodo Rinaldi per la Cura delle Artriti.

By D. Marotta, G. Lazzarini and A. Calò. Ministero dell' Interno Istituto di Sanita Pubblica, Viale Regina Margherita, 299, Roma. $10\frac{1}{2} \times 7\frac{1}{2}$; 38; 1937 (paper).

BIOCHEMISTRY

Annual Review of Biochemistry. *Volume VI.*

Edited by James M. Luck. Annual Review of Biochemistry, Stanford University P. O., Calif. $5.00. $8\frac{3}{4} \times 6$; ix + 708; 1937.

It is always a pleasure to note in our columns this *Annual Review*. The topics in the volume (28 in number) have been well chosen and, as always in previous issues, have been presented by outstanding authorities. Of great value are the extensive bibliographies that accompany each section. It is the policy of the editors to include each year a few reviews on subjects of timely interest and in this volume two such are given—one on the "Application of microchemistry to biochemical analysis" by P. L. Kirk, of the University of California, and the other on the "Biochemistry of fish", by C. M. McCay, of

Cornell University. With this volume an important innovation has been started: namely, the inclusion of a subject as well as an author's index. This adds immeasurably to the usefulness of the book—particularly to those who do not belong in the ranks of the biochemist, but who, for one reason or another, have need to catch up on recent developments in this field.

Mécanisme des Réactions Fermentaires. *Son Étude sur l'Amylase et l'Invertine.*

By L. Ambard and S. Trautmann. Masson et Cie, Paris. 35 francs. $9\frac{1}{2} \times 6\frac{1}{2}$; 103; 1937 (paper).

The idea developed in this book is that in the action of a ferment upon a substrate the total reaction time is a sum of the durations of time required for three consecutive processes. The authors present their results of studies on the factors which account for the variation in the rapidity of these phases when amylase or invertin is used. There is no bibliography.

Annual Review of Biochemical and Allied Research in India. *Volume 7, 1936.*

Society of Biological Chemists, India, Bangalore. Rs. 2 or 3s. (postage extra). $8\frac{3}{8} \times 5\frac{3}{8}$; 165; 1936.

SEX

Sexual Power.

By Chester T. Stone. D. Appleton-Century Co., New York. $1.50. $7\frac{3}{8} \times 5$; vi + 172; 1937.

Sex Life in Marriage.

By Oliver M. Butterfield. Foreword by Sophia J. Kleegman. Emerson Books, New York. $2.00. $7\frac{3}{8} \times 5\frac{1}{4}$; xxi + 192; 1937.

Sex in Religion. *An Historical Survey.*

By G. Simpson Marr. George Allen and Unwin, Ltd., London. 7s. 6d. $7\frac{1}{4} \times 5$; 279; 1936.

Sexual Power contains the customary chapters on impotence, psychic basis of sexual difficulties, physical basis of sexual difficulties, etc., forming an excellent rehash that does not appear to differ noticeably from its predecessors on the same subject.

It seems to us that one of the reasons why so many of the sex books are as inadequate as they are is because the authors were high-minded persons, or rather high-minded persons of the variety who believe that sexual intercourse should be a strictly altruistic enterprise on the part of both of the participants. In the second place, the invariable rule has been to devote a great deal of space to the elements of sexual calisthenics, and neglect the intermediate and higher branches entirely. Thirdly, it is assumed that the reader is a singularly unimaginative, not to say, stupid person. As an illustration of the latter point we quote from *Planning the Honeymoon*, a chapter in Mr. Butterfield's book, as follows: "The best bed is one that is not too hard, nor yet so soft that it wiggles and shakes all over when one moves a hand or foot. If one of a couple is accustomed to a very soft and the other a moderately hard bed, or if one needs many covers and the other few, it will be necessary to compromise a little to arrive at a satisfactory arrangement."

In the introductory chapter of *Sex in Religion* appears the following:

> "There is a feeling abroad today, a feeling which is steadily growing in intensity, that the Church has failed in her duty in so far as she has not dealt fairly with this relationship between sex and religion; and there is a growing desire amongst real friends of the Church that the various problems raised by sex at the present time, including the problems of marriage, divorce, and birth control, should be faced frankly and fearlessly by those in authority."

The sex factor in human affairs is traced from its manifestations in primitive religion to the viewpoints that the author regards as characteristic of modern times. While it is true, as the author so ably illustrates, that the sum total of happiness could be measurably increased by modifying the sex *mores*, it is a little difficult to believe that the increment would be as large as envisaged here. Perhaps too much consideration has been given to the lamentations of sex reformers, many of whom give every indication of believing that there are no maladjustments other than sexual.

"Under the freer and happier condi-

tions such as exist to-day Keats might have lived a very different life"—and might also have written bad poetry.

La Greffe Testiculaire du Singe à l'Homme. *Technique opératoire. Evolution Histologique. Manifestations Physiologiques. Extrait de "Technique Chirurgicale", No. 4, Juin, 1937.*

By Serge Voronoff. Doin et Cie, Paris. $9\frac{1}{2}$ x $6\frac{1}{4}$; 20; 1937 (paper).
[English translation of above under title "Testicular Grafting from Ape to Man." Pp. 15.]

For any graft to be successful, the site of implantation should be as nearly identical to the original location of the tissue as possible. The author therefore decided some years ago that testicular tissue should if possible be placed within the scrotum of the receiver. Since there is considerable injury if an incision is made in the testicular pulp, the grafts were made to the external surface of the parietal layer of the tunica vaginalis. The technique is described in detail in this volume.

Many of those grafts have survived for as long as six years and living sperm have been seen in the tissue several years after transplantation. The author claims that "blood pressure invariably declines in cases of hypertension," that general tonus is raised, that the skin becomes firmer and more elastic, and that there are many other favorable changes both of the nervous system and of general metabolism which last for about six years when "grafting should then be repeated." Similar transplantations of simian parathyroid glands are likewise successful and a case is reported in which a young man suffering from tetanus was cured for a period of eight years.

From the general biological standpoint, the fact of long survival of the simian tissue and the mingling of its cells with those of the human species is more interesting and startling than the claimed physiological results. All the experiments in grafting of animal tissue show that autografts usually succeed and that homografts frequently succeed but that heterografts always fail to persist. In some cases grafting between very closely related species like the hare and the rabbit can be accomplished, the term homograft being applied to this type by Dartigues. Does the long survival of the tissues of higher apes in man then indicate closer relationship than is generally believed to exist?

Réunons Médico-Chirurgicales de Morphologie. *Morphologie Générale et Spéciale, Chirurie Réparatrice et Plastique, Endocrinologie—Dermatologie, Médicine Sportive et Hygiene Orthopédie—Physiothérapie, etc. Comptes Rendus des Séances, Année 1, No. 4, Décembre, 1936.*

Dr. Claoué, ½9, rue Scheffer, Paris. 50 francs (in France); 100 francs (outside of France). $9\frac{1}{2}$ x 6; 89–179; 1936 (paper).

Contains interesting paper (pp. 92–152) on sexual mutilations by Henri Allaix.

BIOMETRY

Five Place Tables. *Logarithms of Integers, Logarithms and Natural Values of Trigonometric Functions in the Decimal System for Each Grade from 0 to 100 Grades with Interpolation Tables.*

By P. Wijdenes. P. Noordhoff, Groningen. Fl. 2.50. $9\frac{1}{4}$ x 6; 168; 1937.

To the astronomers of the valley of the Euphrates we owe the usual method of measuring angles and their subtending arcs, a method based on the sexagesimal division of the sextant. A somewhat improved method which has never enjoyed the popularity which it merits, based on the decimal division of the quadrant, was proposed by the same Congress that formulated the metric system.

The present work is a series of tables of natural and logarithmic trigonometric functions based on this latter system with one hundred grades to the quadrant, subdivided into desigrades, centigrades, and milligrades. There are also supplementary tables for the interconversion of degrees, grades, and radians.

While the present reviewer is glad to

concede the superiority for all purposes of the grade over the degree, he cannot feel optimistic about the chances of acceptance of these tables. The average man is a creature of habit, and is likely to continue measuring his angles, if any, with degrees because his ancestors have always done so.

Principles of Medical Statistics.
By A. Bradford Hill. The Lancet Limited, London. 6s. $8\frac{1}{2} \times 5\frac{1}{2}$; vii + 171; 1937.
The substance of this book appeared in the form of a series of articles recently published in *The Lancet.* They were written for the express purpose of giving physicians a clear idea of the principles of statistical analysis. The enthusiasm with which the articles were received is sufficient testimony of their merit. Without doubt the author has achieved his purpose. He has limited himself to a description of the simplest analytical procedures but these should be sufficient for the general run of clinical investigations. The exposition is clear and involves nothing in the matter of mathematics which could not be understood by the reader of average culture. The sections which in particular seem praiseworthy are those in which the author discusses the selection of samples and certain common fallacies of statistical application. As an introduction to statistics this little book may be highly recommended.

The Design of Experiments. *Second Edition.*
By R. A. Fisher. Oliver and Boyd, Edinburgh. 12s. 6d. net. $8\frac{3}{4} \times 5\frac{1}{2}$; ix + 260; 1937.
Professor Fisher's well-known book *Statistical Methods for Research Workers* deals not only with statistical methods but with the problem of planning experiments so as to furnish as much information as possible on the questions which the experimenter wishes to answer. This book deals with the latter question in more detail. It is often stated that in an experiment all of the independent variables but one should be held constant. As Fisher shows, and others have been aware before, this is not only often impossible but undesirable. By proper planning of an experiment just as reliable information may be obtained regarding the effect of several variables on the dependent variable as could be obtained for only one independent variable.

Mathematische Methoden der Biologie *insbesondere der Vererbungslehre und der Rassenforschung.*
By Friedrich Ringleb. B. G. Teubner, Leipzig and Berlin. RM. 8.80 (In Germany); RM. 6.60 (Outside of Germany). $8\frac{1}{4} \times 6$; vii + 181; 1937.
This excellent textbook of biometry gives in comparatively brief space an exposition of graphic methods, centering constants, variation, probability, mathematical foundations of genetics, Bernoullian, Gaussian, Poisson, and Lexis distributions, and correlation.

PSYCHOLOGY AND BEHAVIOR

The Questioning Mind. *A Survey of Philosophical Tendencies.*
By R. C. Lodge. E. P. Dutton and Co., New York. \$2.75. $8\frac{1}{2} \times 5\frac{1}{2}$; vii + 312; 1937.
This book is an unusually simple and clearly written introduction to some of the traditional topics of philosophy. It is perhaps too simple to be used as a textbook, but it is admirably suitable for the delectation of a reader who is unprepared or disinclined to struggle with a more profound treatment of philosophical problems.

Beginning with the notion that philosophical contemplations can emerge from reflection upon any kind of event or activity, scientific or mundane, the author proceeds to the consideration of epistemology, ethics, the mind, self, and education. These topics are approached from the positions of realism, idealism, and pragmatism. On the whole these three points of view are treated fairly, although occasionally the author expresses a bias. For example, the realistic approach to

ethics is described as an attempt to construct a system of human conduct, with the aid of mathematical logic, which will be a purely formal and abstract science of social physics, a mathematics of ethics. The result, according to the author, presents society as an hedonistic dog-eat-dog affair, and so abstract as to be unconnected with the actual life of human beings in social relationships.

Now it may be true that the logical analysis of ethical concepts is an ideal of certain realists, though assuredly not of all, but even for the few this desire has not been realized. Moreover, a scientific approach to ethics may well turn out to be the best way in which to study problems of considerable importance to our understanding of man.

In spite of these occasional unsatisfactory interpretations of standard philosophical positions, this book serves its purpose admirably well.

FEEDING BEHAVIOR OF INFANTS. *A Pediatric Approach to the Mental Hygiene of Early Life.*

By Arnold Gesell and Frances L. Ilg. J. B. Lippincott Co., Philadelphia. $4.50. $10\frac{1}{4} \times 6\frac{3}{4}$; ix + 201; 1937.

The data presented in this volume supplement those of the previous publications by the senior author and his associates. Richly illustrated, this book contains a detailed account of the development in the feeding behavior of some 10 children in the first two years of life. Following a brief outline of the method of study, the essential feature of which is to make movie records of the infant in its own home environment, the authors describe the motor mechanisms involved in suckling, mastication, and swallowing. There follows a summary of the observations on feeding behavior with breast, bottle, and cup and spoon in the successive periods of the first year of life, and on the patterns of infantile reactions to the presentation of food when this is desired and when it is not. The third part of the book is dedicated to problems of adjustment to feeding and sleeping schedules and of training to feeding and hygienic habits. The broad experience of Gesell is here manifested by the sane outlook with which these problems are discussed. At all times the authors stress the importance of the individual and note that training schedules should be to some extent modified by the individual behavior characteristics which in turn are intimately related to the physiologic development of the infant.

This as well as previous publications constitutes a fundamental contribution towards accumulating objective information relative to infant behavior. It deserves to be read not only by pediatricians, psychologists, and human biologists but should be made available also to the layman since it is written in a clear and straight-forward manner. In an appendix are presented the detailed records of four children. There is a comprehensive bibliography and an index.

IN THE REALM OF MIND. *Nine Chapters on the Applications and Implications of Psychology.*

By Charles S. Myers. The University Press, Cambridge; Macmillan Company, New York. $2.50. $7\frac{1}{4} \times 4\frac{3}{4}$; 251; 1937.

The nine chapters of this book are modifications of as many public lectures given by the author before numerous British Scientific Societies over a period of 7 years extending from 1929 to 1935. The nature of the book deprives it of any unity save that derived from the fact that all the discourses are organized around the modern trend in applied psychology. Dr. Myers discusses such topics as the human factor in accidents; the use of psychology in the choice of a career; a psychological regard of medical education; and psychological concepts in other sciences with an understanding and a simplicity that can come only with many years of keen observation and intensive study.

For the student or professor of general psychology, for the vocational advisor, and for the personnel director of industrial establishments alike, this volume will be both stimulating and enlightening. It is indeed unfortunate that a book of this

caliber should be devoid of a more complete table of contents and an index.

THE PARENT-CHILD RELATION: *the Psychological Background and Other Papers. Individual Psychology Medical Pamphlets No. 17.*
By H. G. Baynes, S. H. Lubner, A. C. Court, M. Marcus and F. G. Crookshank. C. W. Daniel Co., London. 2s. 6d. $8\frac{1}{2}$ x $5\frac{1}{2}$; 71; 1937 (paper).

This journal is the organ of the *Medical Society of Individual Psychology* in London. The late Alfred Adler was its honorary president and its purpose is to make the facts of individual psychology readily accessible to physicians in general practice.

The leading article is a discussion of the relationship of parent and child from the psychoanalytic point of view. In discussing this problem, the author spends a considerable part of time upon the incest problem of Hamlet, the life of D. H. Lawrence, and a novel by Franz Werfel. Supplementing the article is a discussion of childish dreams and their unbelievably deep significance. In addition, there is a series of testimonials by English physicians praising the benefits of individual psychology in their practice.

To the reader interested in individual psychology, the psychoanalytic technique, and the literary approach to science, the journal will be of interest.

FACTORS IN RAT LEARNING. *An Analysis of the Intercorrelations Between 34 Variables. Comparative Psychology Monographs, Volume 14, Number 3.*
By Charles L. Vaughn. The Johns Hopkins Press, Baltimore. 75 cents. 10 x $5\frac{3}{4}$; 41; 1937 (paper).

Ten different set-ups were used in the experiments, including three types of mazes. Four measurements were taken to represent the rat's performance in each situation: the number of entrances into culs-de-sac (errors) on a given number of trials; number of trials to meet a criterion; time spent in correct pathway for a given number of trials; and the time per error for those trials. Measurements of different aspects of the rat's behavior in the various situations, combined with age, weight losses, etc., make up the set of 34 variables. The factor analysis technique was used in working the data.

THE COOPERATIVE SOLVING OF PROBLEMS BY YOUNG CHIMPANZEES. *Comparative Psychology Monographs, Vol. 14, No. 2, Serial No. 68.*
By Meredith P. Crawford. The Johns Hopkins Press, Baltimore. \$1.50. 10 x $6\frac{7}{8}$; 88; 1937 (paper).

The aim of this investigation was "to discuss what natural modes of attack young chimpanzees might employ when presented with certain problems for cooperative solution, and to train them in the solution of such problems." The technique was to train each chimpanzee individually to perform certain manipulations with boxes, etc., and then alter the set-up to such an extent that it would now require the cooperation of two chimpanzees to carry out the same operations.

HYPNOTIC POWER: *Its Cultivation, Use, and Application to Psychotherapy.*
By Colin Bennett. E. P. Dutton and Co., New York. \$1.50. $7\frac{1}{2}$ x $5\frac{1}{4}$; 158; 1937.

This work is largely a description of hypnotic states, with advice to the inexperienced hypnotist as to their production, and their therapeutic use. It is not a treatise on hypnosis. It consists of thirty-one loosely connected chapters and no index.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

STUDIES IN HAND-READING.
By Charlotte Wolff. Preface by Aldous Huxley. Alfred A. Knopf, New York. \$3.00. $8\frac{3}{4}$ x $5\frac{3}{4}$; xvi + 154 + 62 plates; 1937.

Dr. Wolff is a palmist, and also a physician, psychologist, social worker, and, incidentally, an admirer of Freud and Jung. All these accomplishments are supposed to make the lady a "scientific" palmist.

In this book she analyzes from their hands the characters of sixty people from servant girls to church dignitaries. Among the celebreties whose palm readings are included are T. S. Eliot, Bernard Shaw, and Anna May Wong. For every subject an imprint of the hand has been photographed as well as a small sketch illustrating only the most revealing characteristics.

In the manner of the old time practitioners of her art and mystery Dr. Wolff examines first the shape and proportion of the whole hand and then the character of the various lines on the palm. But instead of reading in them fortune, adventure, and maybe a future brunette lover, she sees such high toned things as "underdeveloped ovaries" (shown by lack of moons on the nails) and "sublimated erotic desires" (revealed by the ring of Venus).

In the preface Aldous Huxley enthusiastically says: "Nobody who has had a sitting with Dr. Wolff, for example, can doubt her ability to make a diagnosis of physical conditions and tendencies that is often astonishingly detailed and accurate; can doubt her power to specify past events and date them correctly to within a few months; can doubt her knack of describing character, and the secret springs of action with a penetrating and often disturbing insight." [Reginald, the Office Boy, says that right off he can name two people who would be able to pump up that much skepticism, sitting or no sitting.]

However, Huxley carefully avoids saying whether Dr. Wolff's interesting character studies are actually derived from a study of the hand alone, or with the aid of a lively imagination and a keen feminine intuition, or just possibly the merest smidgen of previous knowledge of the character to be unveiled. But such a low suspicion is unworthy; "scientific" palmists of course always take particular pains to keep from knowing anything about customers until they hold their hands.

Literate ♀ gypsies will find this a useful book.

Typing Academic Papers. *A Manual and a Model for the Author and Typist.*

By W. K. Cunningham, Jr., and Ben M. Patrick. Edwards Brothers, Ann Arbor, Mich. \$1.30. $8\frac{1}{4} \times 5\frac{1}{4}$; xii + 118 + 6; 1937 (paper).

A useful guide, especially for the beginner. Like all such books which do not pretend to be all-inclusive there are omissions—yet this reviewer gleaned bits of information from its 100 or so pages that he had been unable to find in more comprehensive treatises.

VOL. 13, NO. 2 June, 1938

THE QUARTERLY REVIEW of BIOLOGY

DIURNAL RHYTHMS

By
JOHN H. WELSH
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REGULARLY recurring changes in the physical environment, such as are due to the seasons, the lunar month, the tides, and day and night, have resulted in the establishment of biological cycles. These are evident in the tendency for animals to migrate or hibernate, as reproductive cycles, and as tidal and daily rhythms of various kinds. In the normal environment these physiological cycles are kept in operation by the recurring external events, but in certain instances it is evident that they have been so well established within the organism that they may persist in the absence of the normal external stimuli. Many daily rhythms, normally kept in operation by day and night, are known to persist when the plant or animal is maintained under constant external conditions. It is with various types of such persisting diurnal rhythms in animals that we shall be concerned in this review.

The term "diurnal rhythm" is somewhat misleading for, although it is commonly used to designate a 24-hour cycle or something recurring each day, it is occasionally employed to distinguish a phenomenon which is evident during the day as opposed to one which is evident during the night, a "nocturnal rhythm". If we persist in speaking of diurnal rhythms we must be cautious in employing the word "diurnal" to designate that phase of the rhythm or cycle which occurs during the day. For example if one is discussing the behavior of an animal which is normally active during the day and quiescent at night, it would be less confusing to speak of the active period as the "day-phase" than to say that the animal has a "diurnal activity rhythm". Such terms as nocturnal, auroral, diurnal, and vesperal have been suggested to describe the active periods of different animals by Carpenter (1932) and others. At times such terms are convenient but the other common meaning of the word diurnal, "recurring every day", must be kept in mind. It would doubtless save much further confusion if we applied the term "24-hour cycles" to variable phenomena in plants and animals which have a period

of twenty-four hours, but since the term "diurnal rhythm" is in common use it will be employed in this review and considered to have the same meaning as "24-hour cycle".

Daylight and darkness have distinctly different effects on many biological processes, as for example on the rate of photosynthesis in plants. The process of photosynthesis obviously cannot continue in constant darkness. There are doubtless many such phenomena associated with day or night, which are rhythmical under natural conditions and operate in 24-hour cycles, but which are completely dependent on changes in the external environment. On the other hand, as an example from the plant kingdom, the nightly folding of the leaves of the *Mimosa* or "sensitive plant" persists for days when the plant is kept in a constant environment. Among animals, light production, color change, retinal changes, metabolic and general activity rhythms may persist for long periods of time in the absence of changes in the external environment, on which they are normally dependent. Numerous studies of persisting diurnal rhythms in animals have been made and various suggestions have been offered to explain the persistence, but in no single instance has there been a satisfactory experimental demonstration of the complete chain of events which keeps the internal rhythm or cycle operating. In the sex cycle the demonstration of the interdependence of the pituitary and gonads and the cyclical changes in the pituitary helps greatly in our understanding of this complex process. Nothing as definite as this is known about diurnal rhythms. It seems desirable, however, to bring together the results of various investigations and the suggestions offered to explain the findings.

LIGHT PRODUCTION

Most animals which produce light, other than those of the deep sea, do so only at night. Bouvier (1922) discusses this periodic luminescence and other "vital rhythms" in a most stimulating manner. He cites the work of Dubois on the West Indian elaterid, *Pyrophorus noctilucus*, or "cucujo". This was one of the first demonstrations of the persistence of rhythms. Dubois kept these insects in constant darkness, at a nearly constant temperature, and found that they became active every evening at the same hour and began to produce light. Buck (1937) finds that males of the firefly, *Photinus pyralis*, ordinarily flash in the evening between 7 and 9 o'clock. Kept in continuous darkness they do not flash. If after being in darkness they are illuminated with light of a low intensity they flash provided they have been in the dark for 24, 48, 72, or 96 hours, but they fail to flash if they have been in the dark for 12, 36, 60, or 84 hours. When males are kept in darkness and exposed to weak light before the end of 24 hours they do not flash until the sum of the time spent in darkness and the time spent in weak light is equal approximately to 24 hours. It is clear that there exists in this firefly a diurnal periodicity which is manifested by periods of flashing which recur at 24-hour intervals and which persist for at least 4 days in the uniform environment of the darkroom.

Certain phosphorescent copepods were kept in constant darkness for as long as 12 days by Moore (1919). They showed increased activity and light production between 6 P.M. and 5 to 6 A.M., but no light was produced during the day. Moore offered the following explanation.

> The process may, for example, be due to a secretion by certain cells which phosphoresces as each drop is produced, and this process of secretion may

have a period of rest during the day and activity may be timed daily under ordinary conditions, and regulated by alternation of light and darkness. During the day there would be storage in the cell, and at night discharge. On the removal of the stimulus of light during the day this state of alternation of rest and action might persist for a long period.

Crozier (1920) performed a similar experiment on *Ptychodera* and *Glossobalanus*, two balanoglossid worms of Bermuda waters, and found that they would maintain for eight days a clear-cut diurnal rhythm of light production while remaining continually in the dark. The reciprocal experiment could not be made for even at night the expulsion of luminous materials was promptly inhibited by illumination.

Many other observations have been made on luminescence but not on its regular recurrence under constant experimental conditions.

MOVEMENTS OF EYE PIGMENTS AND RODS AND CONES

The various pigments of the arthropod eye, and the pigment and rods and cones of the vertebrate eye, may assume quite different positions at night from those found during the day. The movements normally occur daily at the time of sunrise and sunset and are occasioned by the changes in light intensity. They serve to regulate the amount of light reaching the photoreceptors. Such movements may persist in continuous illumination or darkness; therefore they are controlled, in part at least, by cyclical changes within the organism.

Kiesel (1894) was probably the first to observe persisting diurnal rhythms in the movements of pigment in the eyes of arthropods. He found that *Plusia gamma*, a noctuid moth, when kept in the dark continued to exhibit periodic changes in the eye for several weeks. At night the eye would "glow" and during the day would appear black when illuminated briefly. This was found to be due to the changing positions of the distal or iris pigment and Kiesel believed these movements were associated in some way with periods of sleeping and waking. Demoll (1911, 1917) confirmed Kiesel's observations and suggested that the evidence favored the view that the pigment cells were in some way under the control of the nervous system, for he thought the persisting periodicity was a nervous phenomenon. Demoll was well aware of the failures which had attended attempts to demonstrate motor connections to the pigment cells, but nevertheless believed that some kind of tonic impulses kept the pigment in the dark position, and that narcosis, sleep, or death, all of which brought about a movement to the light position, did so by removing this nervous influence.

The first observations on daily rhythms in the movements of pigment cells of crustacean eyes were made by Welsh (1930a). Under continuous illumination the distal pigment cells of *Macrobrachium*, a Cuban shrimp, were found to move distally at about 6 P.M. and remain in this position until sunrise the following morning. The proximal pigment did not exhibit this periodic movement. Anaesthesia and the cutting off of the circulation to the eye both prevented the characteristic distal movement of the pigment cells at the time of sunset. Thus it seemed that both the nervous system and some blood-borne material were involved in the persistence of the rhythm and it was concluded "that the direct control of the distal pigment cells is by way of the blood, and that the persistence of a diurnal rhythm under constant illumination parallels a periodicity in metabolism which is controlled by the nervous system". The earlier

work of Perkins (1928) and Koller (1928) had already shown the importance of hormones in controlling the chromatophores of crustaceans, and Bennitt (1932a) had demonstrated an interrelationship between the eyes of crustaceans, which strongly suggested a blood control of the retinal pigments. Bennitt (1932b) soon followed with a demonstration of a diurnal rhythm in the proximal pigment of *Cambarus* which were kept in constant darkness, and suggested that "Such a change is most likely to be brought about by a metabolic periodicity in the animal, acting through the circulation". Diurnal rhythms in the movements of one or more of the three sets of pigment cells of the eyes of several species of Bermudan shrimps and prawns were found to occur in continuous illumination or in constant darkness (Welsh, 1935); thus it appears that the phenomenon is fairly general among crustaceans.

The work of Kleinholz (1934, 1936) adds greatly to our understanding of retinal pigment control in crustaceans. He found that extracts of the eye stalks of various crustaceans, when injected into *Palaemonetes* while keeping them in the dark, brought about migration of the distal and reflecting pigments to their light positions. The active substance probably comes from the "blood-gland" (later the name was changed to "sinus-gland"), located by Hanstrom (1934); this will be discussed further in connection with diurnal rhythms in color change. The conclusive demonstration of the endocrine control of eye pigments in crustaceans, and the apparent importance of the nervous system in diurnal rhythms led the author (Welsh, 1936) to postulate either a rhythmic secretory cycle in the activity of the sinus gland, which continues under constant external conditions, or a cycle in nervous and general metabolic activity which affects the activity of the sinus-gland, hence the eye pigments. That there is apparently a detectable difference in the amount of active substance present in the eyestalks of *Palaemonetes* adapted to light and to darkness was shown by Kleinholz (1936) and later by Abramowitz (1937).

That persisting diurnal rhythms in the movements of retinal pigments of crustaceans are due to some well-established internal cyclical mechanism was shown by Welsh (1936) while working with *Anchistioides*, and more recently in work on *Homarus* and *Cambarus*, which has not been published. Crayfish have been kept in continuous darkness except for brief periods when observations were made, and at constant temperatures. At 7°C. the diurnal rhythms, evidenced by the covering and uncovering of the reflecting layer of the eye, have been observed to continue for about five months. At 21°C. they disappear after about four months.

Pigment movements in the eyes of living arthropods such as crustaceans are easy to observe (Welsh, 1930b); hence such forms are ideal for observations on retinal changes over a period of time. This, unfortunately, is not true of pigment and rod and cone movements in the eyes of vertebrates, for here only after the study of sections of the eyes of a number of animals is it possible to establish the positions of these retinal elements at any given time. This may account for earlier failure to observe diurnal rhythms in vertebrate retinas, or it may be that such are not marked or common in occurrence. In the eyes of the catfish, *Ameiurus nebulosus*, where the rods and cones are very large and their movements considerable, Welsh and Osborn (1937) have found that movements occur, characteristic of day and night when these fishes are kept in constant darkness. A few other fishes which were

used in a preliminary study show similar but less marked differences between "day-dark" and "night-dark" retinas.

Although it seems unnecessary to look further for obscure environmental factors which might be responsible for persisting daily rhythms Horstmann (1935), while repeating observations similar to those of Kiesel and Demoll on moths, apparently eliminated electrical changes in the atmosphere as a factor which might be responsible for diurnal changes in the eye.

COLOR CHANGE

Just as light production and retinal changes are physiological processes which are easily observed so is color change, when it occurs. Hence the activity of chromatophores has frequently been used in studies of nerve, hormone, and drug effects. Animals which change color by means of chromatophores may appear very different after they have been kept in complete darkness in comparison with their appearance when kept on a dark but illuminated background. Also the degree of dispersion of the pigment in the chromatophores, at night, may be quite different from that during the day. When certain animals are kept in constant darkness, and at a constant temperature, these day and night differences in coloration may persist for some time, thereby indicating the persistence of an internal cycle which is quite independent of environmental conditions. Gamble and Keeble (1900) were among the first to report a persisting periodic color change. *Hippolyte varians*, small crustaceans, were kept in the dark or in light for several days, undisturbed except for very brief periods when observations were made. The animals took on a nocturnal coloration in the early evening and maintained this until early morning when they assumed the color characteristic of the daytime. During the night the tissues were more transparent and the rate of heart beat was nearly twice as great as during the day. [Recent observations on *Hippolyte* (Kleinholz and Welsh, 1937) failed to confirm the independence of its pigmentary responses, and it seems probable that light conditions were not constant in the earlier work.] Further studies by Keeble and Gamble (1903–04) extended these observations to certain species of *Macromysis*, and the evidence for periodic changes in the general metabolic rate were strengthened by measurements of the acidity of the tissues. These were rather crude measurements made with litmus, but they revealed the interesting fact that liver and muscle became acid in the late afternoon and returned to an alkaline condition the following morning. The blood was always on the alkaline side, but their measurements were such as not to reveal pH changes which may have occurred. These workers believed that these physiological changes, which paralleled a change in color, might be responsible for the activity of the chromatophores but they failed to establish a definite relation. Their belief that a rhythm in color change was already established in the newly hatched Zoea larvae of *Palaemon squilla* seems hardly to be warranted from the data.

Schleip (1910) observed a periodic color change in *Dixippus morosus*, a phasmid, which was correlated with day and night and which persisted for a week in continuous darkness, but he offered little by way of explanation of the phenomenon. His observations were soon followed by similar observations on *Idothea*, an isopod, made by Menke (1911) who found the rhythm to persist for as long as 60 days when this animal was maintained in continuous darkness. Menke was also able to reverse the rhythm by lighting animals

at night and keeping them in the dark during the day. Following nine days of this they were then placed in the dark and the pigment cells were found to be contracted by day and expanded at night, the reverse of the normal condition. This reversed situation held for about a week. *Idothea* was found to have a periodicity in metabolism such as was seen in *Hippolyte* and Menke concluded that the persisting movements of the pigment in the chromatophores were due to internal changes which accompanied a periodicity in metabolism.

The idea that persisting diurnal rhythms in color change are associated in some way with a periodicity in metabolism, and therefore that the underlying control is essentially a chemical one, was replaced by a new hypothesis presented by Piéron (1914). After repeating Menke's observations on *Idothea*, Piéron, who thought that chromatophores were under direct nervous control, suggested that periodic discharges from certain nervous centers could account for persistent day and night differences in the color of the animal.

Persisting daily rhythms in color changes of vertebrates are apparently none too obvious for there are few records of such in the literature. Pauli (1926) observed this phenomenon in larvae of *Salamandra maculosa* for about one week when the larvae were kept in continuous darkness. When continuously illuminated these animals became dark and remained dark. An artificially induced reversed rhythm was always fainter and disappeared much earlier. *Phoxinus laevis* showed very slight changes in color, correlated with day and night, when kept in constant darkness. On the other hand *Xenopus laevis*, a South African toad, was found by Slome and Hogben (1929) to show marked day and night differences in coloration in the absence of light. They were attempting to identify the mechanism responsible for the contraction and expansion of the melanophores under normal conditions, and paid only slight attention to this periodic phenomenon. However, they say that because the nervous system seems to play an indirect rôle in the color changes of *Xenopus*, the mechanism responsible for the persisting rhythm may be a type of conditioned reflex.

A valuable contribution to our knowledge of color change and diurnal rhythms was made by Young (1935). He found that larvae and adults of *Lampetra planeri*, a lamprey, show very pronounced daily rhythms of color change, becoming pale at night and dark during the daytime. Continuous illumination produces maximal darkening and stops the diurnal rhythm. When larvae and adults of *Lampetra* are kept in total darkness, however, the diurnal rhythms usually persist, though diminished in extent. Young concluded that the melanophores of the lamprey are not under nervous control and showed that the removal of the pituitary resulted in maximal paling. Removal of the pineal complex in the larvae and the pineal complex and paired eyes of adult *L. planeri* resulted in the interruption of the diurnal rhythms and maximal darkening of the animal. This was not the first demonstration of the importance of the pituitary in color change, but it served to direct attention to the endocrine system as an important factor in the persisting daily rhythms of vertebrates.

Although Megušar (1912) and others knew that the removal of the eyes and eyestalks of crustaceans brought about a marked change in body coloration and an interruption of periodic color change, a correct interpretation of the results was not made because the obvious explanation was that nerve stimuli to the chromatophores were interrupted by the operation.

It was not known until the work of Koller (1928) and Perkins (1928) that the chromatophores of crustaceans were under direct hormone control. When this was discovered and when Perkins found the source of the pigment-concentrating hormone of *Palaemonetes* to be the eyestalks, the results obtained by earlier workers were subject to reinterpretation. If one examines the recent work on diurnal rhythms in the color changes of crustaceans, it is obvious that the part played by endocrines is an important one. Hanström (1934, 1935, 1937) and his students (Sjögren, 1934; Carlson, 1935, 1936) have shown that the chromatophore activating substance probably comes from a small gland present in the eyestalks of most crustaceans which Hanström has called the "sinusdrüse". The suggestion mentioned earlier that a cycle in the secretory activity of this gland might account for persisting diurnal rhythms in pigment migration in the eyes of crustaceans led Kleinholz (1937) and Abramowitz (1937) to investigate this possibility in connection with diurnal rhythms in color change. Kleinholz found that *Ligia baudiniana*, a Bermudan isopod, when kept in constant darkness, was pale at night and dark during the day. Injection of aqueous extracts of the heads caused paling of dark animals, so in order to test the idea that there might be differences in the amounts of hormone present during the day and during the night, extracts from the heads of dark and light *Ligia* were made and injected into dark animals. Both extracts were equally effective in concentrating the pigment of the melanophores; therefore Kleinholz concluded "that the diurnal pigmentary activity is not due to a cycle of exhaustion and elaboration of secretory material in the endocrine gland controlling the color changes".

Megušar (1912) had shown that *Uca* (*Gelasimus*) had a day-phase and night-phase in coloration when kept in constant darkness. This periodic change disappeared on removal of the eyestalks. Carlson (1936) confirmed this observation and showed that the paling which remained after the removal of the eyestalks was due to the removal of the sinus-gland. Injection of an extract of eyestalks caused a darkening of *Uca* (*Palaemonetes vulgaris* responds in a reverse manner). In order to test further the suggestion that a secretory cycle might be responsible for the diurnal rhythm in color changes of *Uca*, Abramowitz (1937), after standardization of methods of extracting and testing the active principle of the eyestalk, ran a series of experiments as follows. Eyestalks of *Uca* which had been kept in constant illumination and in constant darkness were removed and extracts were made both during the daytime and during the night. This yielded four sets of extracts which were then injected in equal amounts into test animals. Their effects were all essentially the same. This would seem to indicate that there is a constant amount of hormone present in the sinus-gland of *Uca*, regardless of the time of day, or the conditions of illumination. It is possible that the methods used by Kleinholz and Abramowitz on *Ligia* and *Uca* do not detect small differences which may exist in the content of the blood-gland, or it is possible that during the secretory phase the hormone is being elaborated as fast as it is secreted and an extraction would yield as much active material as would an extraction made during the non-secretory phase. There are also important differences between related animals, and while Abramowitz found no differences between the hormone content of *Uca* which had been kept in the dark and in the light, he did find larger amounts of the chromatophore hormone in the eyestalks of *Palaemonetes*

which had been kept in the light in comparison with those which had been kept in the dark, thereby confirming the results obtained earlier by Kleinholz on this same form. In this connection another important contribution should be mentioned, that of Rodewald (1934–35) who finds that the formation of the melanophore hormone in the pituitary of *Rana temporaria* goes on only under the influence of direct illumination. In the absence of light no melanophore hormone is formed, and after a sufficient period in the dark none can be obtained from the pituitary. In *Rana temporaria* there is no persisting rhythm in color change and none would be likely with such complete dependence of the pituitary on illumination for its activation.

GENERAL ACTIVITY AND METABOLIC RATE

The examples of persisting diurnal rhythms which have been discussed thus far are those involving a particular group of effectors such as luminescent organs, chromatophores, or pigment systems of the eye, and in one instance the movements of a group of receptors, the rods and cones. In several of these studies the particular phenomenon under observation has been said to vary with the metabolic rate or with general activity. Day and night variations in activity and in rate of metabolism have been quite widely studied or observed, and in a few instances satisfactory laboratory experiments have been performed under controlled conditions, so that it is no longer possible to dismiss these diurnal rhythms as due to some uncontrolled day and night differences in the environment.

Among the invertebrates the great majority of studies of diurnal rhythms in activity have been made on insects. Some of the earlier work which is interesting and suggestive, although its discussion here is not warranted, was reviewed by Bouvier (1922) and discussed along with further observations by Rau and Rau (1929). One of the first adequately controlled studies of 24-hour cycles in the movements of insects was made by Lutz (1932). By means of apparatus which recorded automatically any appreciable body or leg movement of the insects, Lutz proceeded to obtain activity records under normal day-night conditions and then in subsequent darkness, and also in darkness after periods of reversed illumination. He summarizes his results as follows,

> Two species of crickets and a subterranean grasshopper showed very definite diurnal rhythms which were continued in constant darkness, temperature and humidity. These rhythms were changed by reversed illumination and then the new rhythms were continued in constant darkness with the following exceptions. The crickets that were subjected to reversed illumination for only a short time showed a tendency to return to the old rhythm after a short time in constant darkness, and the *Stenopelmatus* individual that was inactive during a relatively long period of reversed illumination showed no effect of the reversing when it was subsequently active in constant darkness.

Park and his co-workers began a study of activity rhythms in nocturnal insects and certain other arthropods at about the same time that Lutz began his studies of this phenomenon. They find that there are three types of activities (Park and Sejba, 1935) which they speak of as environmentally controlled, inherent, and arhythmic. As attention has been directed, in this review, to rhythms which persist under constant external conditions, mention will be made of only the "inherent" rhythms which Park has described. Park and Keller (1932) observed under controlled conditions the activity of *Boletotherus cornutus*, a beetle. These animals were found to be much more active at night than during the day and this activity persisted under constant external con-

ditions. They also made the very interesting observation that beetles raised from the larval stage in continued darkness gave essentially the same performance as recently collected adults. Whether this indicates that the behavior of *Boletotherus* is "inherent" in the genetical sense or perhaps had been established in the larvae before they were removed to constant darkness seems to require further investigation. It is an important point which must be studied in forms other than mammals where conditioning *in utero* is possible. Another important aspect of the general problem which cannot be satisfactorily studied in mammals is the effect of prolonged fasting on diurnal rhythms. There are some who believe that diurnal rhythms in general activity are closely associated with feeding periods and result from the regular recurrence of hunger rhythms, but when clearly marked daily cycles persist during starvation periods up to 18 days in length, as shown by Park (1935) for *Spirobolus*, a millipede, this belief seems unjustified. The activity cycles of *Megalodacne heros* (Park and Sejba, 1935) are slightly altered when these beetles are kept under constant conditions, but periods of activity continue to occur during the night and there is little activity during the day.

Daily vertical movements of planktonic organisms have been widely studied under natural conditions (see review by Russell, 1927, and more recent papers by Clarke, 1933, 1934). Esterly (1917, 1919) demonstrated a "physiological rhythm" which he felt was in part responsible for the daily migrations of certain copepods. *Acartia* were kept in tall cylinders of sea water in continuous darkness. During the evening hours there was a marked increase in the numbers of animals in the upper levels, this being the time of day when this organism would normally be moving toward the surface. Because of the difficulty Esterly had in keeping these copepods living, observations could be made for only two days. As far as is known, this type of experiment has never been repeated and the possibility of an independent internal control of vertical movements of copepods and similar forms has not been generally accepted. Further evidence has been obtained from a study of the vertical migrations of deep-water animals in the sea which suggests that an internal physiological cycle may play some part in determining their movements (Welsh, Chace and Nunnemacher, 1937).

Stier (1933) observed a daily rhythm in the number of locomotor waves of the sea-cucumber, *Thyone briareus*. The number of waves of constriction passing along the body was found to increase between the hours of 4 P.M. and 2 A.M. This change in rate occurred when the animals were kept in a darkroom and illuminated only with a dim red light. This seems to be the only instance on record of a diurnal rhythm persisting under constant external conditions in any group of animals lower than the chordates excepting the arthropods.

The greatly increased activity of *Cambarus* which occurs in nature during the night persists for several months when these crayfishes are kept in constant darkness, as do the changes in the eye. There seems to be an endocrine control involved, just as there is in color changes of crustaceans, but this work is only now in progress and further evidence is necessary before this conclusion is warranted. Among the lower vertebrates there are few examples of persisting daily rhythms in general activity; they doubtless occur as do rhythms in color change, but attention has not been directed to them. An investigation of oxygen consumption in fresh-water fishes by Clausen (1936) re-

vealed rhythmic fluctuations, correlated with the time of day, which are indications of persisting activity rhythms. Clausen made hourly determinations of oxygen consumption of fishes kept at a constant temperature in containers which were covered to prevent stimulation. Besides differences in oxygen consumption between different species, he found variations of a regular nature over a 24-hour period. Large-mouth bass, *Huro salmoides*, consumed oxygen at a higher rate between 5 and 8 A.M. and again between 3 and 10 P.M. The black bull head, *Ameiurus melas*, exhibited irregularities in oxygen consumption between 5 and 10 P.M. Clausen states,

> The cause of these rhythmic fluctuations in oxygen consumption is not obvious. The constant environment of the experiments precludes any possibility of stimulation being the cause of these rhythmicalities. If this were the cause it would be impossible to place three fishes of three different species in the experimental chambers at the same time and obtain three different rhythms.

Spencer (1929) reported daily cycles in the activity of other fishes, but in the preliminary account the experimental conditions are not given.

Simpson and Galbraith (1905) and Wetmore (1921) measured the body temperature of birds and found this to be correlated with the times of normal activity and rest. Such nocturnal birds as owls have a higher temperature at night than during the day, while the majority of birds being normally more active during the day have a higher temperature at this time. These measurements were not made while the birds were under constant environmental conditions and the results are reported only because they occasionally appear in the literature on diurnal rhythms.

The experimental study of diurnal activity in mammals has been confined almost exclusively to rodents, and among the rodents the albino rat has received most attention. The nocturnal activity of the rat was recorded by Slonaker (1907), but the persistence of this greater activity at night, when rats are kept in constant darkness, was determined by Szymanski (1918) and Richter (1922). Richter attributed this, in part, to feeding habits. Most studies on periodic activity in rats (see review by Richter, 1927) have been concerned with the 2-hour rhythm which is a hunger response, and the 4 to 5 day cycle in females which has been found to be related to the estrous cycle. Little attention has been paid to daily rhythms. Recently experiments with the albino rat by Stier and Beck (communication) show that the activity rhythm persists for at least 58 days in constant darkness. Young rats born and kept in darkness show a definite diurnal activity rhythm which may have its peak during the daytime. If these young rats are exposed to normal daylight and darkness for one day, and then returned to continuous darkness, the major activity period thereafter occurs during the night.

A remarkably well-defined and persistent daily rhythm in the activity of *Peromyscus*, a forest deermouse, was discovered by Johnson (1926). *Peromyscus* were kept as long as seven months in continuous darkness and still showed a daily periodicity although the active phase was no longer at night. Reversed illumination, in another experiment, resulted in a reversal of activity, and the newly-acquired day-activity had persisted for 21 days when the experiment terminated. Attempts to establish artificial days of sixteen hours in length failed. To quote,

> In neither case was the normal twenty-four hour periodicity of the mice changed into or replaced by a sixteen-hour periodicity, although, as has been

shown, the actual time of activity may be quite readily shifted, the general plan of a twenty-four hour periodicity seems to be of a much more fundamental nature.

Young mice born in darkness, and kept for six weeks with their parents, were found to have an activity period that corresponded with that of the parents, which at that time was not in phase with the period of darkness outside. Johnson believed that the persisting activity rhythm of *Peromyscus* is not produced by, nor dependent upon, environmental conditions, but rather is an expression of an internal physiological rhythm.

The Japanese dancing mouse was found by Wolf (1930) to be more active at night, with one of two main periods of activity between 6 P.M. and midnight, and the second period in the early morning hours. These activity periods were maintained in constant darkness except for slight shifts in the onset of activity. Records were obtained on mice born and raised in darkness, and a marked periodicity in activity was found, although it was not correlated with outside day and night. Wolf recorded feeding and general activity separately and found the feeding periods so regularly distributed over periods of high and low activity that he concluded there was no apparent connection.

Davis (1933) found that increased nightly activity of a species of *Microtus*, the short-tailed vole of England, persisted for twenty-four days when the animals were kept in the dark.

Activity records of bats, when kept in constant darkness, with temperature and humidity relatively constant, were recently obtained by Griffin and Welsh (1937). A species of *Myotis* was found to be normally active for two to three hours beginning shortly after sunset. This activity persisted for four days in darkness, which was as long as this experiment was continued. Records were obtained on two *Pipistrellus* over a period of two weeks. One of these bats showed a daily period of activity in the early evening and a second period of lesser activity usually occurred in the early morning. In the other case there was an interesting correlation between times of disturbance and subsequent activity periods. Records were changed at approximately forty-eight hour intervals, the lights were then on in the darkroom for a brief period, and the bats were taken from the recording cages and fed. This failed to affect in any way the rhythm of one *Pipistrellus* but in the other there was a period of activity soon after feeding, a second period approximately twenty-four hours later, and if recording continued uninterrupted for the full two days, a third period near the end of the second day would appear. These periods were related in no regular manner to solar time but were determined by the period of illumination and feeding. The fact that a second and third activity period might appear, after intervals of twenty-four and forty-eight hours, with no further outside disturbance, indicates the presence of internal cyclical processes operating in periods of twenty-four hours.

Browman (1936) has used the albino rat in studies of activity cycles and finds the peak of daily activity, during normal day-night, or during controlled twelve-hour periods of artificial light and darkness, occurs during the night or during darkness. Reversal of the light and dark periods causes a subsequent reversal of daily activity. Constant artificial light for a period of weeks causes fluctuations in the daily activity rhythms of female rats. Peaks of activity may occur for five to six days during the solar night, then, after one to three days of irregular activity, the peaks occur during the solar day, and so on. Rats in constant dark-

ness, and blinded rats, maintain the rhythm of daily activity with which they enter the dark.

A convincing demonstration of an internal 24-hour cycle of metabolism in rats and man has been made by Werthessen (1936, 1937). Using an apparatus which gave a continuous measurement of oxygen consumption and intermittent determinations of carbon dioxide production, Werthessen proceeded to determine these values during thirty-six hour fasts. Both normal rats and rats which had been trained to eat at twenty-four hour intervals were used. The results were essentially the same. The rate of oxygen consumption and carbon dioxide production was high at the beginning of the run, dropped to a minimum at about the sixteenth hour and then rose to a second maximum near the twenty-fourth hour. Following this, unpredictable variation set in. Experiments on four men, using standard apparatus, yielded essentially similar results. These results are quite surprising in view of the general assumption that the basal metabolic rate remains relatively constant, over a period of time, if conditions are kept constant. They confirm the earlier findings of Horst, Mendel, and Benedict (1934) on the rat.

Finally, one of the most recent studies of rhythmic phenomena in the rat (Hemmingsen and Krarup, 1937) has established the very interesting fact that certain phases in the estrous cycle occur at certain definite times in the diurnal cycle. Under normal day-night conditions the maximum of estrus takes place at about midnight, at intervals of four days. Under reversed lighting conditions the maximum of estrus is shifted to midday. These investigators also found it impossible to establish artificial days of sixteen hours, made up of eight hours of light and eight of dark. They say, "Even after two months a struggle was still going on between the tendency to display the activity in the dark periods and the tendency to have one pronounced period of activity every twenty-four hours." The obvious conclusion from this work is that the same internal mechanism which is responsible for maintaining, in the rat, an estrous cycle of a definite duration with its maximum occuring in the dark may be, in part, responsible for the maintenance of 24-hour cycles of muscular activity.

OTHER MANIFESTATIONS OF DIURNAL RHYTHMS

A very considerable group of observations of diurnal rhythms in animals will be omitted from this review, partly for lack of space, and partly because they do not satisfy the requirements stated earlier that in most instances only those rhythms which have been shown to persist, with external conditions controlled, would be discussed. It is inevitable that some studies which satisfy this requirement may be overlooked.

There are a certain number of additional important and interesting studies which emphasize the widespread occurrence of 24-hour cycles and some of these have been chosen to emphasize the desirability of taking these daily variations into consideration in the conduct of certain experiments which continue over a period of twenty-four hours or longer.

If we examine the field of parasitology we find several instances of modified behavior of the parasite, brought about by the daily cycles in activity of the host. One classical example is the daily migration of the microfilariae of *Wuchereria bancrofti* which infect man, (reported by Manson 1879–81, and studied since by numerous workers) in regions where a night-flying mosquito is the transmitter. These microfilariae appear in the peripheral cir-

culation at night and disappear during the day. Chandler (1930) says, "The stimulus which times the appearance of the embryos in the blood is in some way connected with cessation of activity on the part of the host, for it is gradually reversed in people who sleep by day and work by night; yet sleep itself is not the factor, since the embryos begin to appear before the usual sleeping hours." While the factor or factors which govern the periodicity of the microfilariae are not known, it is fairly clear that it is neither an inherent nor an acquired rhythm in the parasite, but some change which takes place in the blood of the host which stimulates them to migate. Boyd (1929) describes a 24-hour cycle in the reproduction of the malarial parasite of birds, *Plasmodium cathemerium*. Changes in length of day and night, reversed illumination, and constant illumination all modify the reproductive activity of this parasite. Boyd concluded the effects were due to changes in the periods of activity of the host. Boughton, Atchley and Eskridge (1935) in a similar way modified the diurnal oocyst production of the sparrow coccidium, *Isospora*, and likewise concluded that it was controlled by host activity.

The diurnal spermatogenic cycle in the house sparrow, *Passer domesticus*, studied by Riley (1937), and the daily deposits of dentin and enamel in the incisor of the rat, observed by Schour and Steadman (1935) are further examples of physiological phenomena which, in order to be understood, must be considered in relation to the general diurnal rhythm in the activities of the organism as a whole. They appear to be entirely dependent on regular changes in the metabolic activity of the animal in which they are found which may in turn depend on environmental changes. On the other hand, ovulation in the common fowl (Warren and Scott, 1936) is, to some extent, cyclical under relatively constant environmental conditions. With conditions normal most eggs are laid by hens between 7 A.M. and 5 P.M. After one to two weeks of continuous illumination Warren and Scott found that egg-laying was fairly evenly distributed over twenty-four hour periods. Under conditions of reversed illumination most of the eggs were produced during the night. These experiments were performed with the birds in individual compartments. Quite different results were obtained when they were subjected to various conditions of illumination in laying houses to which they were accustomed. Here under continuous red light of a low intensity, which was the nearest approach to continuous darkness, over 75 per cent of the eggs continued to be produced in the day period. In the same laying pens under continuous artificial light a group of birds continued to produce most of their eggs during the day for a period of eighteen days. Artificial light only at night then caused complete reversal by the fifth day, but when this was followed by a second period of continuous illumination the birds returned to day laying with a few eggs produced in the early morning and evening. They say that the birds may have been able to escape from the direct rays of the light during the night, but this would not explain the return to the usual day period of laying after a period of complete reversal. The different results obtained when birds were kept in individual compartments and in the usual laying houses they believe may be ascribed to a "psychological" effect of unaccustomed surroundings. After discussing the rôle played by hormones in the sexual cycle of birds they conclude "Since the effect (of light) on laying is regulatory rather than stimulatory, it seems improbable that the hormone theory would

apply here." This conclusion seems to be unwarranted as there are various processes which are "regulated" by hormones and this control may either be dependent on or partially independent of external regulatory stimuli.

"Time sense" in bees, which has been studied by Behling (1929), Wahl (1933), Kalmus (1935) and others is evidence of an internal rhythm which operates in 24-hour cycles, for bees may be trained to seek food only at intervals of this length. Ants and termites, however, may be trained to seek their food at other intervals (Grabensberger, 1933), although they apparently exhibit daily activity cycles under constant external conditions. The differences between the bees and the ants and termites appears to be in their normal feeding habits; the bees seeking food at times when nectar and pollen are available. Such times are restricted to a definite period of the day. The diurnal periodicity of emergence of *Drosophila* from the puparia (Bremer, 1926; Bünning, 1935) persists after keeping this species through several generations under alternating periods of light and dark of eight hours or eighteen hours duration.

Limited space allows no more than the citation of certain recent reviews which deal with the very considerable literature on diurnal rhythms in man. Studies of diurnal variations in human performance, which have been made primarily by psychologists, have been adequately reviewed by Freeman and Hovland (1934). The review by Kleitman (1929) covers the large literature on sleep which had appeared during the previous decade or more. The recent review by Jores (1937) includes many studies on 24-hour cycles in man such as the glycogen cycle of the liver (see also Forsgren, 1935). So many factors have been found to affect the "diurnal curves" of man that the conclusions are most confusing. Man is a poor experimental animal and, at present, it would seem that a more complete understanding of the internal processes responsible for diurnal rhythms may come first from study of the lower animals.

DISCUSSION

From the studies of diurnal rhythms there are a few important points which deserve special emphasis. It is apparent that they are of relatively common occurrence and are manifest in diverse ways. It is also apparent that they may persist for long periods of time in the absence of changes in the external environment which are responsible for their origin, the most important being light. The periods of the rhythm, or perhaps better the phases of the cycle, may be experimentally reversed by reversed illumination; the lengths of the periods may be temporarily altered by artificial days longer or shorter than twenty-four hours; the regularity of the rhythm may be disturbed by constant illumination, but as long as the animal is left intact there is a tendency to return to a 24-hour cycle when subsequently placed in constant darkness. The presence of some internal physiological process or processes operating in periods of twenty-four hours' duration must be admitted. A complete understanding of all phases of a cycle, in any one organism, is far from being realized at the present time. The mechanism which causes an animal to change color at the time of sunset may be quite different from that which awakens a nocturnal animal such as a bat. The problem is to determine whether, for a given animal, there is a timing mechanism which operates independently or whether there is a series of events which operate in a definite sequence, any one of which is dependent on the preceding step.

Endocrine glands and their hormones play an important rôle in color change and movements of eye pigments. The activity of these glands is influenced by the presence or absence of light, but thus far there is no convincing evidence that there is a persisting cycle in secretory activity under constant external conditions.

The effect of anaesthesia and other indirect evidence such as the ease with which activity periods may be shifted about suggest that the nervous system may play an important rôle in diurnal periodicity. This is more than likely in pigmentary changes where the blood gland and that part of the pituitary which secretes the chromatophore-activating hormone are under nervous control.

Regular daily fluctuations in the rate of metabolism of a fasting animal, kept under constant external conditions, must be determined partly by regular changes in the nervous and endocrine systems; it is possible that these variations in metabolism, in turn, affect the coordinating systems. Instead of some one physiological process being responsible for the persistence of diurnal rhythms there would then be a series of processes operating in a cycle. The evidence at hand at the present time suggests that this may be true although in no single organism has a nervous-endocrine-metabolic sequence of twenty-four hours' duration been demonstrated. Until there is more complete experimental data diurnal rhythms or 24-hour cycles will continue to be explained in the same indefinite terms which were applied to the sexual cycle not many years past.

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WHAT ARE THE GENES?

II. THE PHYSICO-CHEMICAL PICTURE; CONCLUSIONS

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COMPOSITION OF SPERMATOZOA AND NUCLEI

THE spermatozoan nucleus or head is the most concentrated packet of genes known to science, and as such is a subject of especial interest. Following the description by Marcus (1921) the human spermatozoan head has a flattened eliptical shape, about .0038 mm. or 3.8 μ long by 3.2 μ in width, so that lying flat it will cover an area of field that is almost exactly 9 square μ in extent. Its thickness, which Marcus fails to discuss, is not over 2 μ at its maximum point, and it tapers down very greatly in other portions, so that an average thickness of 1.5 μ is a fairly liberal estimate. Multiplying the projected area, 9 square μ by the average thickness, it is evident that the approximate volume of the head may be estimated as 13.5 cu. μ. This volume corresponds to that taken up by 13.5 $\gamma\gamma$ (.0000000135 mg. or 13.5×10^{-9} mg.) of water. The actual weight of one sperm head will be several $\gamma\gamma$ in excess of 13.5, as it is unusually dense for a living structure and behaves as a somewhat heavy body when centrifuged. The dry content of organic matter will lie approximately at 5 to 6 $\gamma\gamma$. Practically the whole of the human sperm head, according to Marcus, is nucleus, there being no terminal perforatorium, such as some spermatozoa exhibit, and only an excessively thin cup-shaped film of non-nuclear material covering the basal portion. A rib-like exoskeleton, detectable by ultraviolet light, claims only a negligible fraction of the whole volume. A small vacuole seems to be the only detectable interruption in the homogeneous interior of the head.

Among other animal species sperm heads are to be found both larger and smaller than in man. That of the fruit-fly, *Drosophila*, so important in genetic studies, has a volume between 0.5 and 0.6 cu. μ (Morgan, 1922). Almost without exception sperm heads differ from usual forms of nuclei in that under the microscope they present the picture of being composed of "chromatin" in a compacted form, largely without the presence of nuclear sap or appreciable masses of any other material than chromatin.

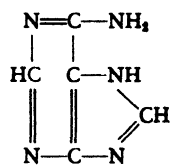
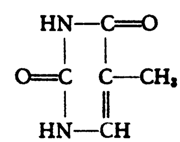
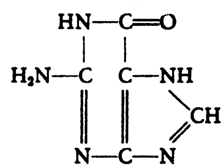
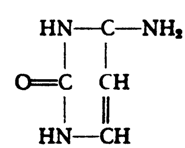
In many forms of animals the entire testicle matures simultaneously, and various biochemists, beginning with Miescher (1874a, 1897), have found it possible to collect and separate sperm heads in sufficient quantity to carry out extensive chemical studies on these practically pure samples of chromatin, the same material, to all external appearances, that is seen in the form of chromosomes when a nucleus undergoes division, and which has repeatedly been identified by geneticists, perhaps a little rashly, as the physical basis of the Mendelian mechanics of heredity. The classic material for these investigations has come from the Rhine

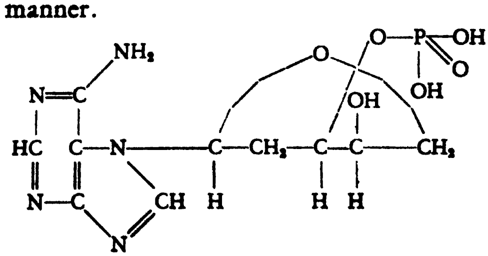
salmon during its spawning migration up the river.

In each species that has been examined, the sperm head chromatin has a surprisingly constant and homogeneous composition. Roughly 96 per cent of the alcohol-ether insoluble solids in sperm heads collected from Rhine salmon consists of a distinctive organic salt known as salmin nucleate, a salt-like combination of thymonucleic acid with a protamine, a very basic protein complex. The structural chemistry of these two ingredients, the acid and the protein, has now been very largely elucidated. (Miescher and Schmiedeberg, 1896; Schmiedeberg, 1900, Burian, 1904, 1906.)

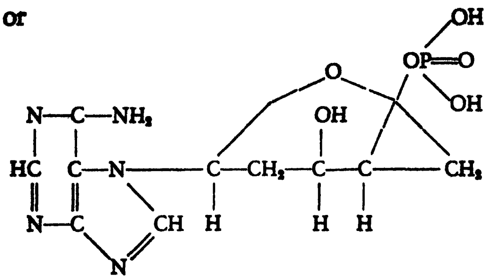
The acid part of the complex seems to be more uniform for various species than the basic portion; being, so far as known, identical in salmon sperm with that from spermatozoa of other fishes, and from mammalian testicle, thymus, spleen, thyroid, liver, kidney, brain, mammary glands, placenta, intestine, etc., and closely related to materials from pancreas and from some bacteria, all of these being structures rich in nuclear substance. (Levene and Bass, 1931.) It is an equimolecular combination of a distinctive sugar with phosphoric acid and a nitrogenous base, with loss of two water molecules. Each molecule contains four such groupings, united through loss of water, entitling it to the name "tetranucleotide." (Levene, 1921.) All four sugars are alike, being identified by Levene and his collaborators as the peculiar pentose d-2-desoxyribose, present apparently in its cyclic form. (Levene, Mikeska and Mori, 1930.) The bases on the other hand are all different, consisting of two purines and two pyrimidines (Table 1). The bonding of each base to its sugar is after the manner of a glucoside, the purines being attached apparently on position (7) (right-hand-NH-), and the pyrimidines on position (3) (lower left -NH-). The phosphoric acid radicle is attached to one of the remaining hydroxyls of the sugar. Thus the adenine mononucleotide should be allowed one of the following structural formulae, the other three component mononucleotides being built in analogous manner.

TABLE 1

PURINES		PYRIMIDINES	
Adenine or 6-amino purine		Thymine or 2-6-oxy-5-methyl pyrimidine	
Guanine or 2-amino-6-oxy purine		Cytosine or 2-oxy-6-amino pyrimidine	



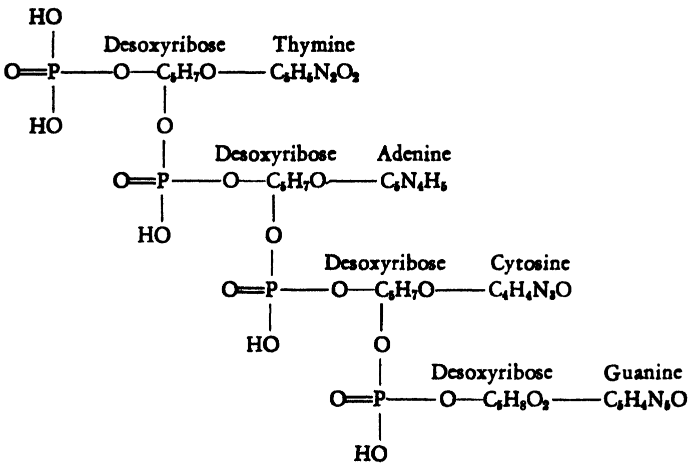
or



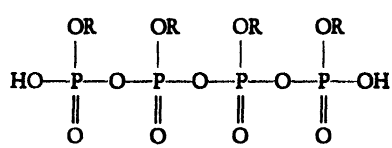
The mode of union of the four mononucleotids to constitute a tetranucleotid is still involved in some controversy. Levene (1921) gives reasons for inferring that it is by an ester linkage between phosphoric acid, and that the purine and pyrimidine mononucleotides alternate with each other.

His titration curves indicate that one of the five acidic hydrogens still remaining is much weaker than the other four. Hence the complete molecule is provisionally represented with the following configuration, the only structural points on which no evidence is as yet deducible being the exact order in which purine and pyrimidine nucleotides alternate, and the question which hydroxyl of the sugar is bound to which phosphoric acid. On this formula there are five acidic hydrogens, one of them weaker than the other four. The corresponding empirical formula is C_{39} H_{51} N_{15} O_{25} P_4, giving a molecular weight of 1253.6.

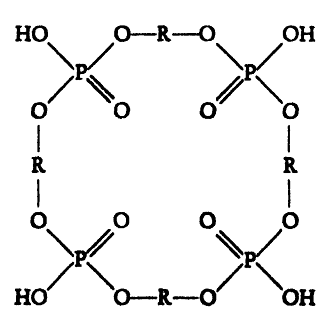
We give below, abbreviated, a formula which was revived by Thannhauser (1934), after it had first been suggested and rejected by Levene. (Klein and Rossi, 1935.) The arguments in its favor are drawn from digestion experiments by Thannhauser. Also a cyclic formula, as indicated below, has been suggested by Takahashi (1932) to accord with his belief that the substance is indigestible to monophosphoric-esterases until started by a diphosphoric-esterase. As neither of these formulae conform to the conditions of ionization indicated by Levene's titration curves, we shall base our further considerations on Levene's tentative formula given herewith.



THYMONUCLEIC ACID (AFTER LEVENE)



Thannhauser



Takahashi

The current impression of a systematic distinction between plant and animal nucleic acids is only valid if we limit attention to the two commonest members of the class. (Levene and Bass, 1931.) A nucleic acid which has been found in wheat germ and yeast differs from the above-described thymo-nucleic acid in having ribose instead of desoxyribose, and uracil in place of its methyl derivative, thymine. But ribose is also found in a specialized polynucleotide of the mammalian pancreas, in the adenine mononucleotide of muscle tissue and in the guanine mononucleotide of various mammalian gland tissues. On the other hand the nucleic acid of tubercle bacilli is credited with desoxyribose and with both thymine and uracil, as well as 5-methylcytosine. The bases in the nucleic acid from the pea are reported to coincide with those from mammalian thymus. (Kiesel and Belozerski, 1934.) Obviously any attempt to state the taxonomic distribution of the nucleic acids is quite premature.

The base to which the nucleic acid is attached also shows noteworthy characteristics. The best known example is the protamine of salmon spermatozoa, which is designated salmin, and is present as the organic salt, salmin nucleate. (Miescher, 1874b; Piccard, 1874; Kossel and Kutscher, 1900; Kossel, 1903; Abderhalden, 1904; Kossel, 1928.)

Hydrolysis shows that the base is a protein composed entirely of four familiar and frequently described amino acids, occurring either in the proportions, 10 arginine + 2 proline + 2 serine + 1 valine − $14H_2O$, or else perhaps 12 arginine + 3 proline + 2 serine + 1 valine − $17H_2O$. These two formulae yield extremely similar figures for the acid-base balance, the former representing a reaction weight (as base) of 188.9, and the latter giving the figure 187.9. On the basis of the first alternative the elementary composition will be $C_{81}H_{155}N_{45}O_{18}$ or a small multiple of this, corresponding to a minimum molecular weight of 2046.6. Its basic character is derived from the free NH_2 groups in the guanidine part of the arginine, and also probably from a residual unattached alpha-amino nitrogen. Each molecule, if we assume the minimum permissible formula, should have 10 strongly basic groupings, and presumably also one that is weaker.

One hundred parts of native salmine nucleate contains, according to Miescher's analyses, 37.35 parts of salmine, the remainder being the nucleic acid (Miescher and Schmiedeberg, 1896). But if allowance is made for a supposed trifling contamination, the per cent will be a shade lower, as pointed out by Burian (1906). These figures fit almost perfectly the ratio of four protamine molecules of the above minimum size, to eleven thymonucleic acid molecules, to make the naturally occurring salt. In other words the average formula should be

$$(C_{81}H_{155}N_{45}O_{18})_4(C_{39}H_{51}N_{15}O_{25}P_4)_{11}, \text{ or}$$

$$C_{753}H_{1181}N_{345}O_{347}P_{44}$$

If the supposed basic and acidic valencies of its components are correct, all of the eleven basic nitrogens will be functioning in each unit portion of the protamine, and on the average four of the five possible replaceable hydrogens will be involved in each thymonucleic acid—44 salt-like bondings in all. The protamine content of this composite molecule would be 37.25 per cent, and its theoretical molecular weight 21976. Allowing an absolute mass of $1.65 \times 10^{-12}\ \gamma\gamma$ per unit of molecular weight, a single such molecule must weigh $3.63 \times 10^{-8}\gamma\gamma$, or approximately $\frac{1}{28000000}\gamma\gamma$.

The species differences in the major composition of nuclei reside much more in the protein part and much less in the nucleic acid component. Thymonucleic acid is almost universally characteristic of the vertebrate series, although certain mammalian glands (e.g. the pancreas) yield related variants. (Levene and Bass, 1931.) The protein constituent of the nucleus may diverge widely in different tissues of the same species, and in the same organ of different species they appear to be identical only in related species or in representatives of closely related genera. (Kossel, 1928.) The entire chemical class protamine is limited, so far as known, to the ripe sperm of certain families of bony and ganoid fishes. The chemical entity

salmine seems to be shared by the Atlantic salmon and some of the Pacific salmons, representing kindred genera. (Yamagawa and Nishizawa, 1934.) Certain other Pacific salmons are suspected of carrying a slightly different protamine. The sperm of the tuna fish has a protamine nucleate much resembling that of salmon, but its protamine differs in containing at least one additional amino acid, tyrosine. In the common carp there seems to be a mixture of two protamines, in one of which lysine, which is no constituent of salmine, predominates over the arginine, while in the other the arginine predominates. The corresponding protamine in sturgeon sperm contains all three of the basic amino acids, arginine, histidine and lysine in such proportions that their sum comprises two-thirds of the total count of amino acids in the molecule. Illustrations of these variations among different families of fishes could be extended further from the literature.

In most animal nuclei other than the ripe sperm of certain fishes, the protein which combines with the nucleic acid is not protamine, but histone. Materials of this class have been reported in chromatin-rich glands, such as calf-thymus, in red blood corpuscles of birds, in the unripe testes of Rhine salmon, in spermatozoa of the cod, and of its relative the burbot (*Lota*), in ripe male organs of several echinoderms, etc. (Mathews, 1897; see also Leipert and Kurokawa, 1933.) These histones are proteins somewhat related to protamines, but with a much larger variety of amino acids, bespeaking a more complex total molecule. Their proportional content of basic amino acids is decidedly less, with the result that their molecule is less markedly basic than that of a protamine. One of the major distinctions of the histones is their digestibility by pepsin as well as by trypsin, while protamines are vulnerable to pancreatic juice but not to gastric digestion.

The histological staining methods by which chromatin is commonly recognized may be looked upon as direct chemical tests for acidic material anchored in insoluble form in the fixed tissues.

Unsatisfied phosphoric acidic bonds in the nucleic acid-protein salts combine either with the basic dye stuff or with the heavy metal of the mordant to produce the highly localized staining effect. In the absence of other insoluble acids this serves both as a confirmation of our conclusion that not all the 5 acidic bonds of the nucleic acid are covered by protein linkage, and as a beautiful demonstration of the localization of these incompletely neutralized nucleic acids in the structures known as chromosomes. But these stains are *per se* diagnostic only of insoluble acids anchored in the tissues, and the interpretation that they spot the nucleic acid is only permissible if it can be reasonably assumed that no other insoluble acids are on hand to participate in the reaction. For intranuclear structures this assumption is at least tentatively justified, although in less known extranuclear structures, it is rather dubious, and in certain cells definitely erroneous. Another inherent defect in the staining methods is the doubtful interpretation when they fail to give a good color. Many nuclei are scarcely touched by "nuclear" stains. The oft-made inference that these cells are nearly devoid of "chromatin," or of typical nucleic acids, may sometimes be correct, but biochemical writers have long recognized the more likely alternative that the nucleic acid has been completely and firmly neutralized by combination with a higher proportion of basic proteins. (Miescher and Schmiedeberg, 1896, p. 126, 149; Neumann, 1898.) It is worthy of note that nuclei showing this condition are sometimes found in cells that are ancestral to the sex cells, which must therefore be carriers of the entire hereditary material in its completeness. Is the histone nucleate omitted from the constitution of such a cell, or has it merely been rendered nonstainable by neutralizing the nucleic acid with a slight relative oversupply of the histone?

Caspersson (1936, 1937) has recently published an intensive study of nuclei, using microchemical methods, ultraviolet photography, digestive technics, etc. He used the giant chromosomes from insect salivary glands, and various other histological materials. The banding which is revealed by staining these chromosomes corresponds very closely to bands of nucleic acid as detected through the fact that nucleic acid is opaque to ultraviolet light of lengths from

2560 to 2750 Å. These bands are a protein-nucleic-acid combination, while the intervening zones are protein that contains no nucleic acid. In cells from other sources he reports a discrepancy between the staining reactions and the distribution of nucleic acid, since this is found both in basophilic and in certain acidophilic cytological structures. During mitosis the nucleic acid is, he reports, more abundant than at other stages, despite the fact that it is absent outside of the chromosomes while the cells divide.

Roughly speaking, there is good agreement between Caspersson's observations and the pictures produced by the Feulgen microchemical test for thymonucleic acid. The latter test rests upon a color reaction for desoxyribose. (Feulgen and Rossenbeck, 1924; Wyckoff, Ebeling, and Ter Louw, 1932.)

The mineral chemistry of nuclei is a subject which has come into much confusion, both as to the data and as to the interpretations. Macallum's belief (1926), that iron is abundantly present in an organic combination in the chromatin, and that the nucleus is essentially free from inorganic salts, is open to several serious reasons for doubt. Such ions as potassium and chlorine are so much more diffusible through dead membranes than are the reagents by which he tested for them, that when we find their precipitates accumulated at or just outside the nuclear membranes the observation will hardly preclude the possibility of a distribution of these ions within the nucleus previous to the cell's death.

Evidence regarding iron in the living chromosomes is complicated by the great avidity of nucleoprotein for inorganic iron, and by the resulting risk that this element will reach the chromosomes during the histological manipulations. (Gilson, 1892.) Unpublished observations by the present author show that even a very long treatment by Macallum's ammonium sulphide histological method generally fails to bring out any coloration of the chromatin, if the preparations have been kept rigorously free from all extraneous iron. But the sulphide reagent is microchemically speaking a sufficiently good solvent so that iron from external sources, such as rust-containing dust, is definitely transported by it into the chromosomes, causing them to develop a good iron-sulphide color. It is clear, then, that even when chromatin takes the stain in a preparation that has been kept free from the accidental introduction of iron, this does not give us any information about the *in vivo* location of the metal. Macallum's hematoxylin test for iron, being even more delicate than the sulphide and ferricyanide tests, is subject in just that much greater a degree to the difficulty that infinitesimal traces of foreign iron may give a spurious positive result. (Macallum, 1897; Dieterle, 1930.) Mühlmann (1928) refined this method by the use of a control slide which received a preliminary treatment with oxalic acid. Structures staining deep blue or blue-black must contain iron if the control slide shows them unstained. But even this can only be useful in chromosome studies if it be true that one can "unmask" the supposed nuclear iron from its supposed organic linkage without permitting it to shift its location.

Micro-chemical incineration tests made by Policard show that spermatozoan heads and many other nuclei yield an abundant colorless ash, as contrasted with the rust-yellow ash obtained from the cytoplasm of cells that are rich in iron. (Policard, 1933, 1934; Policard and Rojas, 1935.)

We judge, then, that chromatin can contain considerable mineral matter. The evidence does not indicate whether this consists of mineral salts, of organically bound material, or both. Iron is not a predominant element in nuclear ash. Indications are that Macallum's nuclear iron is, at least much of the time, an artefact, and that if there is any intrinsic iron in the chromosomes, it cannot be

abundant and cannot be brought out by Macallum's ammonium sulphide procedure.

From various of the findings thus far rehearsed it seems justifiable to infer that all but a trivial residue of the material of the salmon sperm consists of a substance, complex indeed, and chemically labile, but so monotonously homogeneous in its molecular constitution, and so devoid of special characteristics that we cannot avoid the feeling that what is most significant in the physical basis of heredity has quite escaped the chemical analysis (Koltzoff, 1928). Are we to find this significant something in the residual trifling quantities of unanalyzed organic substance, or shall we suppose that the most completely known and simplest of all natural protein compounds is the seat of specialized arrangements of its parts that endow it with the tremendous potentialities of the genetic units,—or must we now turn scientific pessimists and substitute a mystical for a scientific analysis? If we choose to credit the unanalyzed residue as the seat of all that is really crucial in the assembled genes, we must keep it in mind that in each spermatozoon this residue only constitutes about 3 per cent by dry weight, comprising in the case of sperm heads of the human size, scarcely 0.2$\gamma\gamma$ of dry substance, or in common units .000,000,000,2 mg. The corresponding residual matter in *Drosophila* must be 25- or 30-fold less still. The alternative that some hidden character of protamine nucleate may account for the genes is refuted when we find that earlier generations of germ cells in the salmon testicle possess no protamine whatever, but apparently a nucleic acid combination with an entirely different type of protein, belonging to the histones. Thus in addition to its inadequate diversification, the chromatin of salmon germ cells fails to have the chemical continuity that should distinguish the physical mechanism of heredity. Even the variable staining reactions of different germ cell generations serve to remind us that "chromatin" and "gene substance" are not synonymous. The large amount of genetically inert chromatin found in Y-chromosomes points to the same thought. It would be more nearly correct to argue that chromatin is the matrix in or on which the genes are located. (Miescher and Schmiedeberg, 1896, p. 149; Muller and Painter, 1932; Goldschmidt, 1927, p. 99.)

THE NUMBER AND SIZE OF THE GENES

Considering the minuteness of the spermatozoan head, it becomes of interest to ask how many genes it must be supposed to contain, and of how much material they are each composed. No thoroughly satisfactory mode of estimation is known, but the order of magnitude of the figures can be appraised by various methods.

In the common fruit-fly, *Drosophila melanogaster*, more than 400 loci of recessive mutations have been recognized, and the number is still increasing. Of these, upwards of 150 are of the type known as "sex-linked," viz. carried in the X-chromosome. (Morgan, 1922; Muller, 1922, 1929.) It scarcely needs to be mentioned that the total equipment of genes is certainly in excess, and in all probability very manyfold in excess, of those that have come under laboratory observation as the result of mutations.

The chromosome maps, which Morgan and his associates have made, symbolize in chart distances the relative intimacy of gene linkages within a chromosome versus the degree of freedom of cross-over. It is demonstrable that the distance intervening between genes is a major factor in the ease with which they become separated by cross-over into the other chromosome, although probably mere distance is

not the only factor. Thus the maps express a real linear relation, but doubtless with more or less distortion through local stretching and shrinking of the unit of length. If we assume, for simplicity's sake, that there are no such distortions in the map, then it appears that the genes lying closest together among those that have been studied statistically are separated by about $\frac{1}{540}$ of the total length of one of the large chromosomes. (Morgan, 1922.) On this scale of linear units there might be in *Drosophila* about 1600 genes, as the haploid count. The elements of uncertainty render it hard to judge whether this is an over- or under-estimate.

Another method of approaching the problem is by treating statistically the frequency of repeat mutations. (Muller and Altenberg, 1919.) If genes are all equally subject to mutation, the frequency curve that has been found for repeating and non-repeating mutations within the number that have been recorded would correspond to the existence of about 2,000 genes in the haploid count. (Morgan, 1922.) It is probable that this is an underestimate, because the differences in mutability of different genes would distort the frequency curve in that direction.

The curious giant chromosomes found in the salivary gland cells of *Drosophila* have recently yielded valuable evidence upon the general structure of the genetic material. In these cells the strings of genes appear to have multiplied themselves not less than eightfold without the occurrence of fission, and furthermore the homologous chromosomes of the diploid count are joined side-by-side as if in a sort of synapsis, so that each patent chromosome is not less than a sixteenfold structure throughout all its length. In addition, a vast growth of matrix material renders each chromosome 150 or more times as long as it is during the maturation divisions. (Painter, 1934.) When so stained as to bring out only the most acidic portions, a complex structure is revealed, which Bridges (1935) has attempted to interpret.

What appear to be transverse plates of "chromatin" can be resolved in many instances into sets of 16 spherical or clam-shaped vesicles of strongly staining acidic substance, each enclosing an unstained core. Zones which vary from this pattern seem to do so either by attenuation or by intensification and confluence of the original units. In some instances the confluence of one set of clam-shaped vesicles seems to result in a double chromatin plate, guarding both sides of a narrow, clear space. Bridges reports that if the center of each vesicle is the locus of a gene, the initial, haploid count of genes must lie between 2650 and 3540. The range of his uncertainty does not come from variation of the material, but from the difficulty in interpreting double bands.

Working with the same material, Muller and Prokofyeva (1935) made a detailed study of a few loci at the tip of the X-chromosome. They report that single gene translocations correspond to translocations of single chromomeres only in the case of the smallest chromomeres, and that the larger so-called chromomeres are really linear strings of several genes close together. Thus the individual genes in these giant chromosomes occupy particles of chromatin of a size just resolvable by visible light waves. A reappraisal of the chromatin particles, making due allowance for what they see as the composite structure of larger "chromomeres" leads them to multiply the previous estimate of the same giant chromosomes some two or three-fold; viz., to perhaps 5,000 or 10,000 units equivalent to genes.

An investigation by Gowen and Gay (1933) has been interpreted by them as leading to a startlingly high total count of genes.

Using Muller's statistical technique upon their own data from mutations induced by X-rays, they found the minimum figure 1975 for loci of visible, viable mutations, agreeing almost exactly with Muller's minimum of 2,000. But they found in addition to this that dominant lethal "mutations" were 5.5 times as frequent as visible viable mutations, and recessive lethal alterations 7.3 times as frequent. If all of these represent separate gene loci, the total count would be 13.8 times as great as heretofore supposed, or a haploid minimum of more than 27,000 loci. Recognizing, however, that there is likelihood of a great overlap among these three categories, Gowen and Gay suggest that the most numerous single category be used as the basis of an estimate,

and hence that separate genes may be tentatively judged to be 7.3 times as numerous as hitherto supposed, or in their own figures approximately 14,380 for the haploid count in *Drosophila*.

It should be remembered, however, that the high ratio of lethal changes is capable of the alternative interpretation that any one gene when subjected to X-rays may show various alterations, and that among these the lethal alterations are several-fold the most numerous.

In this connection Patterson (1932) studied a few selected loci in *Drosophila*, and reached the result that 87.5 per cent of the X-ray changes occurring at these loci and detectable by his methods are recessive lethal, and 12.5 per cent are visible viable mutations. Data of this kind will need to be increased before they can be used with much statistical accuracy.

However, if we assume that Patterson's ratio of 87.5 per cent lethals is more or less approximated among gene alterations in general, it will follow that the 1975 genes which have been indicated to account for visible viable effects would also be responsible for $\frac{87.5}{12.5} \times 1975$, or 13,825 recessive lethal effects out of the 14,380 to be accounted for, and that the remaining 555 recessive lethal effects, if they are due to gene modifications at all, cannot be cited as representing more than $\frac{12.5}{87.5} \times 555$, or 79 additional genes, bringing the required minimum up to 2,054. The implication would be that these additional 79 represent nothing more than the fluctuations of an inadequate statistical series.

We conclude that in the light of Patterson's ratio, the statistics of recessive lethal effects do not alter our previous estimate for the minimum count of genes in *Drosophila*.

The aggregate length of the chromosomes carrying the haploid supply of genes in the second spermatocyte metaphase of *Drosophila* is about 7.5 μ, so that by Muller's earlier minimum figure (2,000) they must contain not less than 267 genes for each μ of chromosome length, or 1 gene for each 3.75 mμ of the chromosomes. If they are spherical, this would mean a volume of 27.6 cu.mμ. The minimum count estimated by Bridges (2,650 genes) would allot one gene to each 2.94 mμ. Again, Muller and Prokofyeva's figures would call for 1 gene in each 1.5 mμ of these metaphase chromosomes. However, it is argued on microscopic evidence that the chromosomes at this stage have a coiled structure, twisting alternately for short distances in right-hand and left-hand spirals, so that the effective length of the string of genes may be quite a number of times the straight length of the compacted chromosomes. Since the chromosome must be able to split longitudinally into perfect halves, there is presumably a limit to the possible degree of spiraling.

For an earlier elongated thin-thread stage of the nuclear transformation, in which there can be no suspicion of a really close spiral, but in which it is easily possible that the gene particles may be strung along with interspaces, Morgan found (1922) that on the basis of 2,000 genes to the haploid count, occupying 40μ of effective length, there would be approximately 50 genes to each μ of chromosome thread, giving a maximum limit of 20 mμ length of thread per average gene at this state (Muller, 1922, 1929).

If each space of this size is occupied by a spherical gene, these spheres would need to average 4,190 cu.mμ or less in volume, and their total content in a haploid cell could not exceed .00838 cu.μ. Or if the genetic material is better represented by 3,540 spherical beads in a 40μ string, the average diameter could not exceed 11.3 mμ, with the average volume of their individual spheres approaching a top limit of 755 cu.mμ, and their aggregate volume not over .00267 cu.μ per haploid germ cell. Since a sperm head of *Drosophila* has a size of about 0.53 cu.μ, the value .00838 would represent 1.58 per cent of the estimated volume of a sperm head, and .00267 would represent 0.50 per cent in terms of volume. Percentages higher than these are only possible if the transverse dimen-

sions of the gene are greater than the dimension longitudinal to the string—a possible situation upon which we have no evidence, either positive or negative.

From another angle of approach it has been suggested further by Gowen and Gay (1933) that the amount of the gene material can be appraised by mathematical analysis of the effect of X-ray treatment, in terms of quantum relationships.

They assume that every quantum of energy withdrawn from the radiation during passage through the sperm head will result in the radical alteration of the organic molecule within which it was discharged. If we analyze their data we find that an average of 4.4 per cent of the quanta discharged (i.e. 5.5 per cent in the series with copper X-rays, 3.3 per cent with chromium X-rays) produce genetic results, if we include all the lethal and non-lethal varieties of effect. The figures run: Dominant lethal (av.) 1.75 per cent; recessive lethal, $4/3 \times$ dominant lethal, $= 2.33$ per cent; visible non-lethal, $\frac{1}{5.5} \times$ dominant lethal $= 0.32$ per cent; $\therefore$ total of identified mutation-sensitive and any other lethal-sensitive substance $= 1.75 + 2.33 + 0.32 = 4.4$ per cent of the sperm head. The remainder (95.6 per cent) is judged not to be mutation-sensitive, hence to be inert matrix.

The quantities and ratios given above are as stated by Gowen and Gay, but rearranged by ourselves for this presentation.

Gowen and Gay have proposed the tentative hypothesis that this target of sensitivity corresponds to the crucial gene substance.

In that case the total mass of gene substance in the sperm head would approximate $.044 \times .53 = .0233$ cu.μ, in terms of the killed and stained volume. From this it would follow that the average size of the single gene would be

$$\frac{.0233}{\text{total number of genes}}$$

viz. .0000117 cu.μ or 11700 cu.mμ each, if their frequency is 2,000; or on the basis of 3,540 genes, given above as an alternative, the measure would be .00000658 cu.μ, or 6,580 cu.mμ. On the basis of the data presented by Gowen and Gay we are unable to agree to the validity of their estimate of 1.0×10^{-18} cu.cm. (equivalent to 1,000 cu.mμ) since it appears to us that the manner of calculation carries the implication which they themselves reject, of a total gene count in excess of 27,000. Nor is it apparent within the abbreviated limits of their published calculations that they really have three appraisals of the gene size based on sufficiently separate experiments to stand as mutually independent determinations.

The above calculations are open to the more general limitation that gene alteration is not the only way in which a quantum of X-ray energy can affect the chromosomes (Stadler, 1932.) In plant studies a very large share of the cases studied show translocations of appreciable portions of the chromosomes, alterations, that is, which may well be referable to damage of the supporting structures within which the genes are located. It appears from Patterson's test of this feature that in *Drosophila* a rather sizeable minority of the lethal effects may very well be of this nature, and in that case there is nothing in these experiments to preclude a somewhat smaller estimate of the total number of genes, and of the percentage of the chromosomal substance which they comprise.

A new form of tentative calculation of the gene size in *Drosophila* has recently been made by Muller (1935), by using the following assumptions:

In ordinary chromosomes the chromonema threads are conceded to be coiled. The giant chromosomes of the salivary cells appear to have them uncoiled. The X-chromosome has, in this case, a chromonema 200 μ long, of which it is known that only two-thirds—133 μ—can be genetically active (Muller and Painter, 1932.) Since Muller s present figures allow about 1000 genes to this chromosome, his estimate brings an average of 8 genes per μ in what is looked upon as the active portion of the chromonema. (Muller and Prokofyeva, 1935.) Let us assume that the oögonial chromonemata have the same dimensions as those in salivary cells, only disposed of in a spiral form; then if the entire oögonial chromonema of this chromosome were uncoiled to a length of 200 μ, the total bulk of this chromosome could only furnish material for a cylindrical thread this long and .02 μ in diameter. Or if the 200 μ should be drawn as a geometrical spiral line

upon the outer surface of the oögonial chromosome, the laps of the spiral would be spaced at intervals .044 μ apart. So on the basis of the assumptions with which he started, he proposes that genes are not more than some 20 to 40 mμ in transverse diameter, but that on account of heavy spiraling there is longitudinal room on the chromonemata for the genes to be as much as 125 mμ long, or spaced with that large an average interval between their centers. The size of gene which would fill this elongated space is to be looked upon as over-shooting the valid maximum, in-as-much as it assigns 100 per cent of the mass of the active segments of the chromosome to the actual gene substance.

Looking through all these estimates, we are led to a first approximation that the volume of an average gene lies between 28.6 and 4,190 cu.mμ. The larger of these figures cannot be exceeded very heavily for the reason that such large genes would preempt too large a proportion of the chromosomal mass, and leave over too little to be the containing matrix. Judging by the second spermatocyte chromosomes, this maximum is a substantial overestimate, as it seems hardly credible that those chromosomes are sufficiently spiraled to take care of 267 such large particles to the linear μ.

The minimum size reasonable for a gene can be checked upon by the assumption that we should not suppose it to fall below the size of an ordinary protein molecule. Ovalbumin, which counts as a relatively simple protein, has been studied by several techniques, and its molecular size appraised. (Calvery, 1932.)

By count of the amino acids contained, its molecular weight should be approximately 34,000 (i.e. $\frac{1}{17800000}\gamma\gamma$) or a small integral multiple of this amount. By osmotic pressure it has been estimated at roughly 36,000. (Burk and Greenburg, 1930; see also Marrack and Hewitt, 1929.) Ultra-centrifugation indicates for it a molecular weight of 34,500 $\pm$ 1000, a bulk corresponding to a sphere 4.34 mμ in diameter and a specific gravity of about 1.45. (Svedberg and Nichols, 1926; Nichols, 1930; Svedberg, 1931.) The ovalbumin particles are reported to be approximately spherical when in solution, with a spherical volume, as calculated from the above diameter, of 39.2 cu.mμ. Surface tension tests indicate that under surface conditions the particles measure 3.08 $\times$ 3.08 $\times$ 4.17 mμ, which would make them enclosable in a rectangular solid having 39.6 cu.mμ. (duNoüy, 1925.) At the face value of this comparison it appears reasonable to infer that a gene should not be expected to drop below a total volume of 39 cu.mμ. But if we accept the recently proposed lamellar structure for these "globular" protein particles, it is possible that the minimum adequately individualized particle consists of fewer lamellae, and that the smallest reasonable size for a gene would be a flat disk having major diameters of 3 or 4 mμ, and in the other direction perhaps only 1 or 2 mμ. However, opposite faces of protein lamellae are strongly polar, and inasmuch as the gene chain is not longitudinally polar, we are not permitted to picture this chain as a column of protein lamellae stacked up like coins. If 3.08 mμ is the minimum linear space that each gene must occupy, the maximum count in the second spermatocyte of *Drosophila*, with its 40 μ of chromatin thread, cannot exceed 13,000. This is an extreme minimum size and maximum count, there being, as we have seen, no positive evidence favoring any larger number than 5000 in the haploid count in *Drosophila*.

The question may be asked whether particles as small as we have described are adequate for the functions that a gene must perform, and here once more comparison is in order. It is not entirely relevant that some of the hormones have outstandingly small molecular weights, as their significant effects depend entirely on their stimulation of living cells, under such conditions that the complexity of the result seems to be easily referable to the complexcity of the cells receiving the stimulus. Enzymes present a better comparison, because of their frequently great specificity, because of their wonderful power to bring about transformations that could not have been predicted *a priori*, because they can work without the aid of additional biological complexities, such as are necessary for the hormones, and finally because they are heat-labile, which we may presume the genes also to

be. Several of the enzymes show indications of a protein constitution, or of linkage to protein, and the size of their particles or molecules may be inferred to be of the order of magnitude of the proteins. In the case of pepsin, a crystalline preparation which is believed to represent the pure enzyme gave evidence (under ultracentrifugation) of having a molecular weight between 34,400 and 36,600, i.e. quite comparable to crystallized ovalbumin. (Philpot and Eriksson, 1933.)

Viruses, such as the bacteriophages and the active principles of plant mosaic diseases, are particularly tempting for a comparison because they furnish a parallel that comes nearer to true biological conditions. Indeed Gowen and Price (1936) have shown that genes and viruses are much alike in their susceptibility to X-ray irradiation, and McKinney (1937) has shown that they are similar in their propensity for mutation. In drawing a further comparison we must keep in mind that a virus may very possibly be an entity one stage higher than the gene, since it has sufficient biological autonomy to invade successive host organisms, and even to undergo progressive adaptation to taxonomic differences in the host. Unfortunately, the principal methods for measuring ultramicroscopic particles are either doubtfully accurate or very difficult when applied to bacteriophage. Their supposed diffusion constant seems to point to a diameter of about 12 mμ.

The closest grained filters through which they can pass are believed to have maximum pores of 300 to 400 mμ, which on the usual basis would mean a particle diameter presumably between 20 mμ and 50 mμ. (Krueger, 1936.) When subjected to ultracentrifugation one bacteriophage seemed to consist of particles with a specific gravity of 1.11 to 1.14 and a diameter of 79 mμ to 90 mμ, a size equivalent to that of 8,000 or 10,000 molecules of ovalbumin. (See Bechold, 1934; Thornberry, 1935b.) On the other hand a more recent determination made upon another bacteriophage leads to an apparent particle diameter of 11 mμ, or a size only equal to some 16 or 18 ovalbumin molecules. The smaller of these estimates are well within the possible size-range of the genes. The larger appraisals correspond more nearly to the aggregate size of a gene plus its proportionate quota of surrounding chromatin, for if the 530,000,000 cu.mμ (i.e. 0.53 cu.μ) of a *Drosophila* spermatozoon should be divided into 2,000 equal spheres, they would each measure 79.7 mμ in diameter.

The recently accomplished purification of the virus of the tobacco mosaic disease gives opportunity for even more cogent comparisons. (Stanley, 1935a; 1935b; 1937a; 1937b.) This is a crystallizable substance, showing the composition of a nucleoprotein and endowed with powers of enzymatic autosynthesis. (Bawden and Pirie, 1937.)

It yields 16.5 per cent–16.7 per cent nitrogen, has an ash content of about 1 per cent, sulphur 0.26 per cent, phosphorus 0.51 per cent, and gives the biuret test and color tests for the amino acids tyrosine and tryptophane. A temperature of 94° coagulates and inactivates it. Combined with relatively basic proteins (globin, trypsin, trypsinogen, clupein) it is reversibly precipitated, thereby suspending its infectivity. In this reaction its apparent acidic reaction weight is about 4000. If the protein portion is separated from the nucleic acid it becomes pepsin-digestible. The analyses indicate the presence of a tetra-nucleotid similar to yeast nucleic acid, but not stereoidentical. This comprises 5.2 per cent and the protein 94.8 per cent.

The diameter of the tobacco virus particle is stated to vary heavily with changes of the pH and other influences. (Eriksson-Quensel and Svedberg, 1936.) Waugh and Vinson (1932), reported an active preparation with an apparent particle diameter not higher than 5 mμ. Thornberry (1935a) reported an active solution of this virus in which the particles were found to measure about 11 mμ judged by their diffusion constant. According to Bechold (1934) centrifugation of a solution of tobacco virus gave evidence that the particle-diameter was about 50 mμ. Dimensions almost this large are indicated by Stanley's estimate that the molecular weight is 17,000,000, or equivalent to about 500 ovalbumin molecules (Stanley 1937a and b). Bawden and Pirie argue that in its native state the unit particles are in the shape of plates or short rods about 17 mμ in diameter. In this connection it is to be noted that Clark (1938) ob-

served in the dry gel of tobacco virus a fundamental spacing of 15.2 mμ which changed to 21 mμ in the wet gel, and came into the range of 30 to 47 mμ in the liquid condition.

It appears, then, that such viruses as the tobacco mosaic and the bacteriophages, which may conceivably rank biologically one stage higher than the genes, are sufficiently minute to stand in harmony with the provisional conclusions concerning the dimensions of genes.

It is by no means necessary to suppose that all genes are equally small, or that in some phyla they may not far exceed the upper limit of size given by the above calculation made from the germ cells of insects. In various Liliaceae Belling (1931) observed an extremely minute particle at the core of each chromomere, which he believed to be the gene itself. He found from 1400 to 2500 of these particles per cell, according to the species—a very reasonable total number. They take the nuclear stains with especial intensity. Any experimental data bearing upon Belling's suggestion would have the greatest interest, especially as it would be extraordinarily gratifying to biochemists to learn the staining and other microchemical characteristics of plant genes, and to compare them with the gene-like structures in the salivary cells of insects.

In summary, the total haploid number of genes in a *Drosophila* germ cell having an X-chromosome must be not less than 2,000. No explicit evidence has been found in favor of numbers much exceeding 5000, and there are definite reasons unfavorable to a count greater than 13,000. The principal linear dimensions of any individual gene should not be accounted less than the diameter of a "globular" ovalbumin molecule (i.e. 3 or 4 mμ). Their shape remains thus far indeterminate, e.g. whether spherical, discoidal, etc. In *Drosophila* they cannot exceed 4,190 cu.mμ in average size or a sphere 20 mμ in diameter, representing a bulk equal to 107 ovalbumin molecules. Their true average size is probably well within these extremes. In other organisms the size may exceed the *Drosophila* maximum, but it cannot well drop below the cited minimum.

The genus *Homo* has characteristically six times as many chromosomes as *Drosophila*, with an average length of individual chromosomes during most stages somewhat greater in *Homo*. If the average count of genes per chromosome is comparable in the two species, this would allow for man a haploid quota lying somewhere between 12,000 and 78,000 genes. Another mode of comparison depends on the second spermatocytes, for which it is estimated that Wieman's drawings (1917) indicate 12.4 μ as the aggregate length of the 12 chromosomes, which seem here to be duplex structures, making 24.8 μ the length in simplex terms, or 3.3 times the corresponding length in *Drosophila*. If there are comparable numbers of genes per μ of length in the two species, this would give for man a haploid gene count between 6600 and 43,000. Either of these figures seems surprisingly small when we consider the complexities of the human organism, or when we consider that the largest figure gives a diploid count barely in excess of one's theoretical count of ancestors seventeen generations ago.

It has already been mentioned that the human spermatozoon cannot be supposed to carry more than about 0.2 γγ of crucial gene material. Since this figure lies completely outside the scope of common experience, we venture the further comment that it requires on this basis not less than 5 thousand million spermatozoa (a number greater than the world's human population) to carry one milligram of crucial genetic substance.

APPLICATION OF THE ENZYME AND HORMONE CONCEPTS

The nature of the regulation exerted by genes has most often been described as enzymatic. This thought is founded on several considerations. In the first place the disproportion between ultraminute regulatory particles and grossly visible biochemical outcome scarcely permits any other hypothesis than enzymatic catalysis. Then again many Mendelian characters are recognizedly due to loss, alteration, or change in the quantity of certain known enzymes, as, for example, the oxydases that regulate the formation of pigments. (Scott-Moncrieff, 1937.) Also there is the long-standing observation that breakdown of the nuclear membrane, as at the start of the cleavage of the egg, has the effect of touching off a burst of enzymatic activity in the cytoplasm, either from the release of active principles out of the nucleus or from their sudden generation in consequence of the inflow of nuclear substance. Further in harmony with this concept is the observation that embryonic cells, as well as cell masses that regenerate amputated parts, develop in perpetual interrelation, as if influenced by a steady exchange of more or less diffusable regulatory substances.

Substances of the nature of hormones, having, that is, a drug-like potency with effects specific to the type of cells upon which they are working, may also be invoked, as they would conform to many aspects of the experimental observations. (Troland, 1917.) Goldschmidt (1927) in particular has emphasized the appropriateness of supposing a regulation carried out through hormones, emanating from the gene and working sometimes at intracellular and sometimes at intercellular ranges. Such hypothetical hormones may be looked upon as end-products of enzymatic actions set up by the respective genes. Without using this term, Ekman (1930) favors the same fundamental thought, arguing that the cytoplasm (and not the nucleus) is the regular seat of synthesis for the more familiar enzymes.

Both enzymes and hormones are conditioned in their action by the nature of the medium in which they lie. A hormone may produce a dozen different effects in a dozen different tissue types, and in other tissues perhaps no effect at all. Enzymes are notoriously specific as regards substrate, co-enzymes, anti-enzymes, and the like. Thus both enzymes and hormones allow ample room for the requirement that one set of genes shall regulate the Entwicklungsmechanik variously in different parts of the developing organism.

The importance of enzyme regulation is not limited to the formative stages of an organism, and if genes are enzyme regulators we ought to find indications that they function throughout life. In accord with this, it is well known that after intensive activity of any adult gland, nerve, or muscle tissue, the histological evidences of nuclear fatigue are very pronounced. (Hodge, 1892; Dolley, 1909.) If carried to the limit there even results a picture of impoverishment of the nuclear substance, almost to the point of total destruction.

The raw materials, out of which the genes construct the active substances (enzymes or hormones) that they send out into the cell, must be provided by the materials observable within cell nuclei. The most abundant of these is usually histone nucleate. Complex lipins may also play a significant rôle, to judge by modern studies of hormones and vitamines. Spermatozoa and other nuclei undoubtedly possess important lipins, but the actual percentages are not high, and as yet our information about them is very scant. Another possibility is that spare molecules of the gene substance itself may float free

and function as a hormone. (Haldane, 1937.)

As a corollary to the thesis that genes exercise their influence by way of hormones it is theoretically conceivable that the converse relationship may also hold good, and that the gene may be alterable through hormonal or humoral influences from other portions of the body. Some such effect is conceivable either as an alteration of the future reproductive cells, or as a modification of the internal potencies of corresponding genes in different organs of the body. The latter proposition has never been put to an experimental test. The tentative reaction of most scientists is to take the same attitude toward it that is taken toward the question of humoral modification of the traits of offspring, for which the following evidence is the most cogent available.

If a newly fertilized angora rabbit ovum is transferred into the oviduct of a rabbit belonging to the Belgian breed, the resulting offspring shows no trace of genetic influence from the foster-mother, but retains all the angora characters, even to displaying certain distinctive reflexes. (Heape, 1890, 1897.) A still more drastic experiment is to provide a young spayed guinea pig with ovaries from a source representing a different hair color. (Castle and Phillips, 1911.) Under these conditions an albino foster-mother has no slightest diluting effect either on a deeply colored pelt or on one that is already a pale cream (Castle, 1911).

AUTOCATALYSIS

The process by which a gene brings about its own reduplication has been referred to by various authors as autocatalysis. (Muller, 1922; Koltzoff, 1928.) From the mathematician's standpoint the concept of autocatalysis is very precise, but as a biological concept it is quite the reverse. Whenever any reaction, event, or active substance works in a manner to bring forth more of the same reaction, event or active substance, the effect counts as autocatalytic. Processes as diverse as the hardening of linseed oil, a forest fire, and the normal increase of human populations, are all perfectly good examples of autocatalysis, and as such conform to the mathematical rules when the external conditions are held constant. The autocatalysis of genes, which might better be styled autosynthesis, belongs to a special class of such phenomena, and challenges special consideration. The action is in many ways comparable to that of the intracellular synthetic enzymes. It is generally accepted, for example, that synthesis of proteins in the living cell is carried out by means of protease enzymes, which probably are also themselves proteins. Recent literature has, for example, called attention to papain as a protein-building enzyme that is reported to be itself a protein. (Bergmann and Niemann, 1937; Bells and Lineweaver 1937.) Similarly the gene must be an enzymatic particle, most probably protein in nature, or at least protein-like in its framework. One of its enzymatic traits must be to condition the synthesis of its own kind of gene substance. In this respect it is strictly comparable to the viruses, such as bacteriophage and the crystallizable tobacco mosaic virus. The substrate for this reaction appears to be the mixture of protamine or histone nucleate and cell sap extractives that normally environ the genes. The startling situation is that among the thousands of different genes present in any one cell, each severally has the unique potency for producing new molecules that duplicate its own peculiar constitution. Furthermore, when a chemical alteration is suffered through some physical disturbance (mutation) we

have at once not merely a new gene, differing from its antecedent gene by some slight item of chemical constitution, but at the same instant there springs up in it the new potentiality to activate hereafter the synthesis of molecules of this same new variety. (Muller, 1922.)

We are not aware that any other cases outside of genes and viruses have been reported in the field of chemical catalysis, in which there is such perfect specificity combined with what might be called the flexible feature, that any slight alteration in the chemical constitution of the activator molecule leads at once to an exactly duplicate alteration in the synthesis that is induced. It is not necessary, however, to regard this metaphysically as a "vitalistic" manifestation, because in another chemical field—crystalography—there is a perfectly comparable example of selective action (Troland, 1917), molecules being able to join a particular crystal only if they have or take on a structural configuration exactly like those that already compose the crystal. We may say, then, that the autocatalysis of genes must represent a reaction mechanics after the manner of the protein-synthesizing enzymes, combined with a regulative feature in some way akin to what is observable in the forces that regulate the formation of crystals, with their molecular specificity. Koltzoff (1928) proposes, for theoretical reasons, that it is a general trait of the unspoiled proteins of the living cell to tend to catalyse the formation of duplicate molecules to their own. If this hypothesis is verified, then genes and viruses become special cases of a much more wide-spread type of chemical activity.

The peculiar dead-stop which this process suffers as soon as the number of gene molecules has been doubled is another feature that could easily be related to some sort of crystalographic or aggregational limitation. There is, at least, no evidence that it is due to exhaustion of material. We note the suggestive fact that the same structures which evince this autocatalytic dead-stop upon reaching the duplex condition show the further peculiarity during the maturation divisions, of pairing off, like-to-like, with their counterparts in the homologous chromosome. Thus there seems to be something special about the duplex condition in genes that awaits further elucidation.

Life in general has been described as autocatalytic. But it should be noted that self-propagation of cellular or multicellular structures is essentially a derived attribute springing from the autocatalytic and other catalytic activities of the included genes, conjoined, possibly, with the autocatalytic propensity of the cellular proteins. Experimental studies indicate that outside of genes the only visible formed structures with a comparable degree of self-propagating power are the plastids of plant cells. (Muller, 1922; van Wisselingh, 1920; Ekman, 1930; Renner, 1934.) It seems scientifically superfluous, then, to postulate any additional reproductive potentialities for the larger units of life.

HOW FAR CAN CHEMISTRY PICTURE A GENE?

Our study of the chemistry of the germ-cell nucleus did not reveal the material of the genes, but only the matrix in which they lie, from which their substance must be compounded. From this study of the material background of gene construction, we may infer that the chemical framework of the genes must be protein in nature, with a wealth of the more basic amino acids, and that also they may well contain substances derived from nucleic acid. Insect salivary cells give evidence of a concentration of nucleic acid at or around the locus of the gene, but there is no clear basis on which to determine

whether nucleic acid enters into the gene constitution, or whether it serves simply as a mechanical matrix or as a metabolic source for smaller structural units, such as purines and pyrimidine rings.

There is such a high degree of congruity in the behavior of all genes that we must allow to their protein molecules a very strong family resemblance. We do not know whether it is essentially protamine, or histone, or a member of some quite different division of the proteins. Their structure must, however, be vastly more complex than any protamine that has been studied, so great, indeed, that there is practically no limit to the possible number of new gene substances that can exist within the bounds of their chemical family; just as, for example, there seems to be no limit to the number of members in the hemoglobin family of proteins.

The molecule characterizing a specific gene must have a very definite chemical and space configuration, which remains perfectly constant except during the process of mutation. Theoretically any slightest change of the molecule must count as a mutation.

During the last few years much has been learned about the internal atomic pattern upon which proteins are constructed. (Jordan-Lloyd, 1937; Linderstrøm-Lang, 1935.) According to X-ray and related studies there appear to be four major types of protein molecule, as follows:

(1) The linear polypeptide, as exemplified by clupein, (See Fig. 1) the protamine from herring spermatozia; (2) The fibrous proteins (Astbury and Woods, 1933), (See Fig. 2) having a characteristic laminated grid pattern that can be analyzed by X-ray; (Bernal and Crowfoot, 1934; Crowfoot, 1935; Wyckoff and Corey, 1936). (3) The colloidally dissolved proteins with massive, more or less "spherical" molecules, in which a laminated internal structure may be detected; (Clark and Shenk, 1937; Clark, 1938.) (4) A possible polyhedral type of large molecules, formed by a different arrangement of the lamellae. (Wrinch, 1937b.) Other patterns than these four are not necessarily excluded. In both the non-fibrous and the fibrous laminated proteins the principal plane of each lamella is occupied by the carbonyls, α-carbons and α-amino groups of the peptide linkages, apparently bound into hexagons, very nearly after the manner that Abderhalden long advocated. The remainders of the constituent amino acids project away from the principal plane like the nap of a velvet. The spacing between the principal planes of the lamellae approximates 1 mμ or 10 Å.

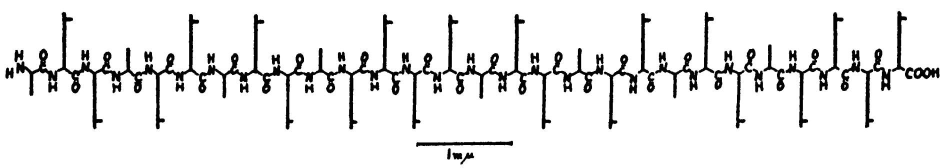


FIG. 1. MOLECULE OF CLUPEINE, OR HERRING PROTAMINE, ACCORDING TO LINDERSTRØM-LANG
Long vertical strokes represent arginine residues; shorter vertical strokes, residues of other amino acids

X-ray analysis of the fibrous proteins (e.g. keratin) leads to the conclusion that their lamellae have an elongated hexagonal ground pattern. (Astbury and Woods, 1933.) This design is one that is geometrically equally possible in relatively small protein molecules and in fibrous masses of indefinite extent. Figure 2 represents a limited grid of this fibrous type, redrawn from Astbury's and Woods' diagram for keratin, making use of considerations brought up by Wrinch (1936b) and by Rainey (1937). The projecting "nap" of amino-acid residues lies on both sides of the principal plane.

The lattice-work found in hydrated and globular protein molecules has been examined in certain cases.

Astbury and Lomax (1935) describe a three-dimensional pattern built by laying straight polypep-

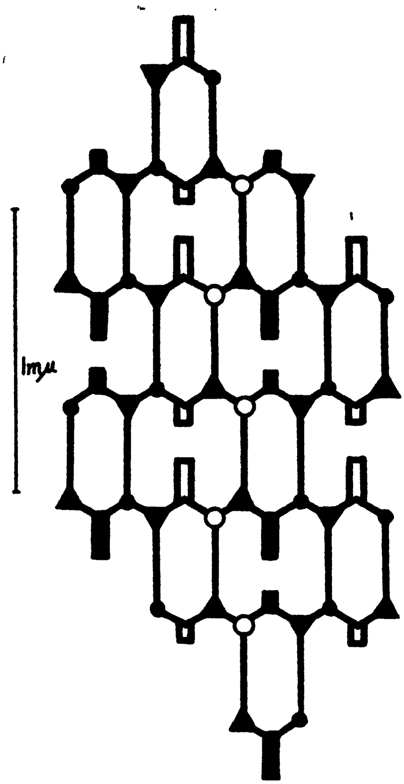


Fig. 2. Pattern of a Fibrous Protein Lamella, Keratin Type, Essentially after Astbury and Woods

Symbols

Amino-acid residue, pointing obliquely out and up.

Amino-acid residue, pointing up.

Amino-acid residue, pointing obliquely out and down.

Amino-acid residue, pointing down.

NH group.

N—without H.

C=O group.

C—OH hydroxyl pointing up.

C—OH hydroxyl pointing down.

tide chains side by side. The polypeptides are judged to carry one amino acid residue for each .35 mμ of their length, the residues projecting alternately on opposite sides of the chain. In the plane of the residues these polypeptides are conceived to engage each other through the residues, being held apart at a distance of 1 mμ or 10 Å between chains. At right angles to this plane the chains are repeated at a grid spacing of .45 mμ.

Non-fibrous proteins can be found with molecular weights all the way from a few thousand to several million, but a molecular weight of 34,500 ± or integer multiples of this occur with such persistent frequency in the most diverse proteins as to demand especial attention. "Spherical" molecules of this size show diameters of 3.5 mμ to 4.2 mμ, indicating a thickness equivalent to four (or possibly three) lamellae. If the amino-acid residues in these proteins vary in average weight from 120 down to 114, it would follow (on the basis of four lamellae) that there must be some especially stable configuration of lamellae occurring at a molecule size in the range of 280 to 320 amino acids.

Bergmann (1937) contends that this Svedberg unit of protein molecular weights (roughly 34,500 ±1000) is due to a fixed normal content of 288 amino acids, or an exact multiple of this, in the native proteins. This number may be expressed as $2^5 \times 3^2$. He believes that in general the number of residues of any one amino acid present in the total molecule is also such that the number can be given the form $2^n \times 3^m$, where n and m are integers (or zero), and that in the main the ratio of any one amino acid is a fraction $\frac{1}{2^n \times 3^m}$ of the whole count of residues. That is, each amino acid will represent something like $\frac{1}{72}$, $\frac{1}{32}$, $\frac{1}{24}$, $\frac{1}{18}$, $\frac{1}{4}$ or some similar fraction of the entire number. To account for such a frequency-pattern, one can hardly suppose otherwise than that there is a precise, rhythmic internal structure to the large molecule, and to

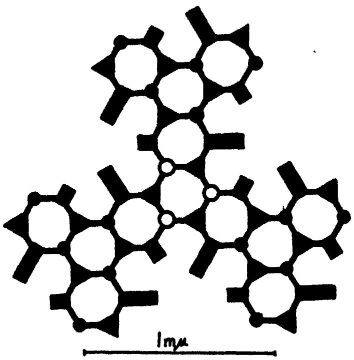


Fig. 3

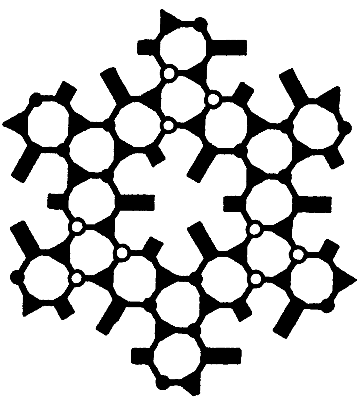


Fig. 4

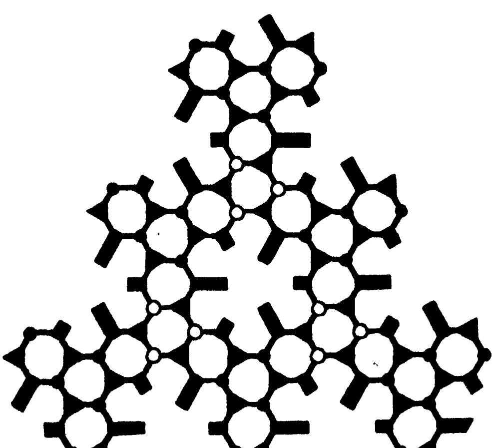


Fig. 5

Figs. 3, 4 and 5. Examples of Supposed Cyclol Configuration, with 18, 24 and 36 Amino Acids Each

Symbols are the same as in Fig. 2

this end Bergmann postulates that each lamella of the protein molecule is built as a much coiled flattened condensation of a polypeptide chain in which the respective residues recur at regular intervals.

Wrinch (1936b) has presented arguments in favor of a "cyclol" pattern of grid for these proteins, as illustrated in Figs. 3, 4, 5, 6 and 7. This pattern can yield three series of symmetrical figures which are respectively hexagonal, triangular and biaxial.

Our drawings show representatives of each of these, a hexagon with 24 amino acids, triangles with 18, 36 and 48 respectively, and a biaxial figure with 72. Among the most stable configurations should be 24, 38, 58, 78, 84, 98, 142 (more stable than 144), 180, and 276 (more stable than 288). If the Bergmann number series is valid, the cyclol units that are most available as lesser units in the protein molecule are the figures with 18, 24, 36 and 48 amino acids (Wrinch, 1937). One could conceive of submolecular lamellae built out of these groups in flat clusters of fours or threes. Flat clusters like this, with a size of 72 to 98 residues to each lamella, would represent diameters of about 2.8 mμ to 4 mμ.

Due to the laevo-configuration of the α-carbons the cyclol pattern brings all the "nap" of the "velvet" on one side of the principal plane. (Jordan-Lloyd, 1932.) Our figures illustrate this point, and represent the absolute orientation as worked out by Rainey (1937). The other face of the lamella is free from bulky obstructions, and is provided with numerous hydroxyls produced by the shift of hydrogens onto the carbonyl groups. Such a free face should be capable of specialized contact reactions with other compounds possessing condensed hexagonal groupings, such as vitamin-D, sex hormones, carcinogenic hydrocarbons, and various alkaloids.

Clark and Shenk (1937) discuss another possible variant of lamellar protein, which was first propounded by Astbury and Woods (1933) and which they believe

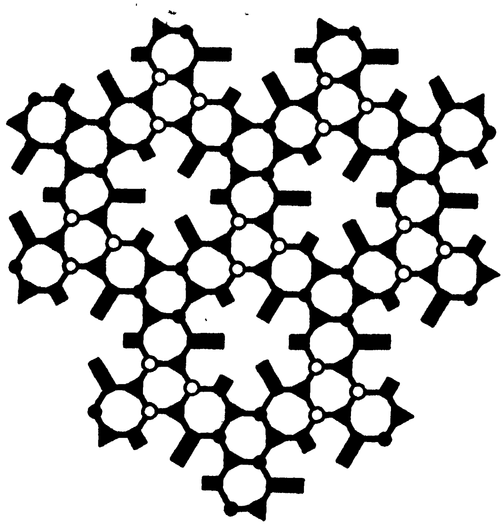


FIG. 6

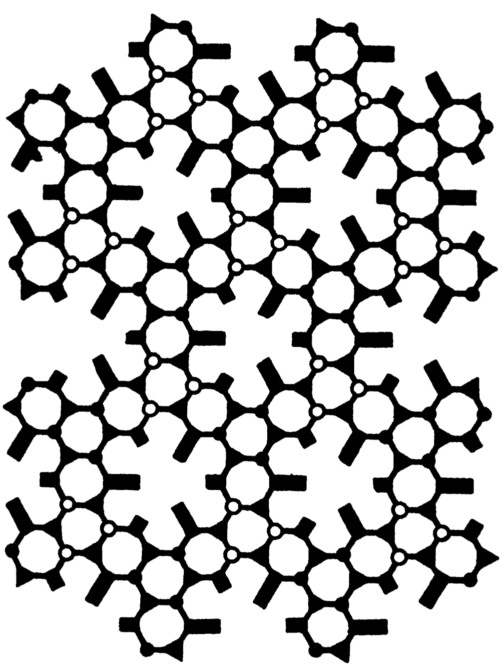


FIG. 7

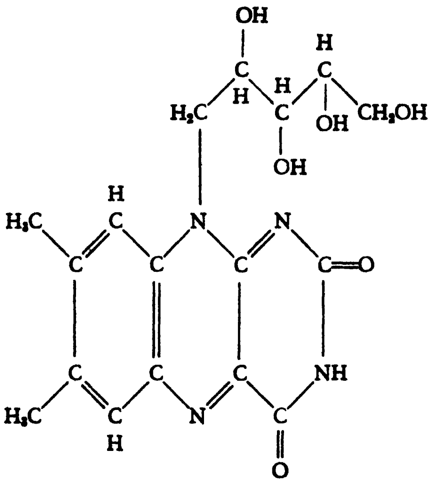
FIGS. 6 AND 7. EXAMPLES OF SUPPOSED CYCLOL CONFIGURATION OF PROTEIN LAMELLAE, CARRYING 48 AND 72 AMINO ACIDS EACH

comes near the true condition of egg albumin. In this the fundamental spacing has much in common with fibrous proteins, but the polypeptide strands are thrown into a serpentine, within the limits of their lamella.

The lamellae of a protein may either be stacked like a layer-cake, or joined into a hollow polyhedron by meeting at an angle along their edges. An example of the latter type has been proposed by Wrinch (1937b) as the shape of the insulin molecule, an octohedron, namely, with a cyclol grid of 36 amino acids on each face, or 288 in all.

If we look upon the genes as comparable to enzymes, it becomes appropriate to inquire into the modern studies of enzyme molecules. (Northrop, 1937.) Pepsin and trypsin are reported to be "spherical" molecules very nearly the same size as ovalbumin, viz., with a molecular weight of about 34,000. The digestive enzymes for carbohydrates are credited with a molecule that falls apart into a distinctive, specific, possibly non-protein portion, bound chemically onto a non-specific portion which may differ even in the same enzyme, but which must have a colloidal character and is doubtless generally a protein. (Willstatter *et al.*, 1922; Waldschmidt-Leitz and Reichel, 1932.) The specific portion is represented as taking on molecules of the substrate, which thereby become susceptible of synthesis and hydrolysis. This two-parted structure has been postulated as a general characteristic of enzymes. Evidence for or against the generalization is scant, but there is at least one further example to be found in the "yellow oxidative enzyme", which is a union of a protein with "vitamin B_2", a specific active principle having a flavin structure. (Warburg and Christian, 1933; Euler and Adler, 1934.) The nucleus of the flavin residue is particularly well

shaped to superpose geometrically upon the hexagonal grid of a protein.



Thus from the analogy of enzymes we gather the suggestions that a complete gene molecule may have a molecular weight of comparable magnitude to what is prevalent among the water soluble proteins, and that involved in the molecule there may be a non-protein specific "prosthetic" group, or groups, very possibly having a condensed cyclic structure such as will superpose effectively upon the hexagons of the protein grid.

If the biological reduplication of genes, called for by cell division, is to be explained through chemical forces, it is requisite that the existing gene should have its parts so laid out in space that the new parts which must go into the newly constituted gene can be laid down upon the old as a direct pattern, each constituent close enough to its counterpart in the already existing gene to come within range of the chemical forces which must draw it into proper relative position, as if for crystal formation. (Gulick, 1937; Haldane, 1937.)

This requirement is not met satisfactorily by any hollow polyhedron, but is met admirably by the flat lattice configuration, with either one stratum, or a definite not too large number assembled in layer-cake form. The new lamellae needed during growth could then be accumulated either externally, by lateral accretion, or internally, by intussusception. Cleavage would occur by delamination, either just before or just after the additional lamellae had formed, provided, of course, that there must at the moment be an even number of lamellae, to allow of an equal and homologous cleavage through the mid-plane. It must needs be inferred that some particular small number of lamellae functions as a stable phase, which tends to be restored as soon as possible after each of the interruptions that are brought about by growth, cleavage, and conjugation.

We note that there are at least the following possibilities among lamellae-shaped molecules for the gene:

(1) the keratin or fibroid type of grid, including the Astbury and Lomax variant; (2) the cyclol grid; (3) a lattice built by the combination of protamine-like electro-positive fibers lying parallel in one direction bound together by salt formation with nucleic acid constituting electro-negative fibers running transversely to them; (4) and (5), respectively, fibrous or cyclol grids interlaminated with nucleic acid molecules to neutralize their diamino residues. It is probable that the third of these alternatives corresponds to the structure of the indifferent chromatin in the spermatozoa of the herring. Wrinch (1936a) has accordingly suggested that genes may consist of short segments—possibly short cylindrical shells—having this structure. As it appears to us that Wrinch's argument springs from too close an identification of genes with the transient protamine nucleate found in ripe spermatozoa of a few bony fishes, we hesitate to give precedence to this alternative over the others in the list, which latter conform to the structures of more complex proteins as worked out by Wrinch, by Astbury and Woods, and others.

It has more than once been proposed that the linear chain of genes can be explained in terms of bundles of parallel and similar protamine-like peptide molecules, perhaps each held together by nucleic acids, but maintaining their linear order by virtue of end-to-end salt-like linkages between the terminal carboxyls and amino groups of the peptides. For such a picture it is necessary to suppose that the identical peptides, built up in juxtapo-

sition, shall have all their carboxyls side by side at one end and all their free alpha-amino groups similarly at the other end. This compels an electrical polarity hardly compatible with the observation that segments of chromosomes in *Drosophila* and various plants may without harm be turned end-for-end within their respective chromosomes. (Kossikov and Muller, 1935.) The conclusion is almost forced upon us that the longitudinal binding must be by nucleic acid micels, whose opposite ends are both electro-negative, and that genes must be protein kernels of a type that can present electro-positive poles in both directions, namely either basic "globules" or lamellar proteins, or arrangements of protamine-like filaments lying transversely to the chromonema.

A suggestion by Caspersson (1936) is that the gene consists of a protein nucleate, the protein constituent being responsible for the diversity of genes, and the nucleic acid contributing to the non-specific internal mechanics of the genes. The zones of unconjugated protein (free from nucleic acid) found alternating with the chromatin nucleic acid in insect salivary cells he would not consider to be gene substance, although he does not claim that all nucleic acid is limited to genes. This viewpoint is in good agreement with Muller and Prokofyeva's interpretation of their observations on single-gene translocations.

The presumable parallelism between genes and viruses requires us to pay a little further attention to the data that we have cited for tobacco virus. If the Svedberg weight unit for the protein part of this material is 35,700, then each 2 Svedberg units (carrying 6 atoms of sulphur) are combined with 3 molecules of tetranucleotid. As the X-ray diffraction shows a strongly marked spacing of 15 $m\mu$ to 17 $m\mu$, and none in the range of 3–4 $m\mu$ it appears that the fundamental particle size must be some higher multiple of the Svedberg unit. (Bawden and Pirie, 1937). There is also indication of a strong lamellar spacing at 1.1 $m\mu$, but no lines in the neighborhood of .45 $m\mu$ and .35 $m\mu$ are outstanding enough to carry much significance (Wyckoff and Corey, 1936; Clark, 1938). A long list of further diffraction lines are shown, but no more detailed interpretation into three dimensions has been proposed thus far. The molecule evidently has a somewhat intricate lamellar pattern involving both the protein and the nucleic acid ingredients. By analogy it increases the likelihood that we shall eventually learn that genes are nucleoprotein molecules.

The process of gene mutation would appear in any of these pictures as a minor chemical alteration occurring at some spot in one lamella or in an attached prosthetic group. Delamination must then produce dissimilar genes, and if all the lamellae regenerate their own type, the two kinds of genes will soon both be established on a permanent basis.

Thus we find that the provisional sketch which chemistry can draught for a gene is not without its vivid tentative details. But we must keep scrupulously in mind that very nearly every item in the suggested space-relations still awaits confirmation or revision in the light of future experiment. We believe, not that science has solved these problems, but that it is now just coming within striking range of the question of the actual make-up of genes as chemical structures.

ARE GENES LIVING?

This humanly natural question stands somewhat in risk of becoming the starting point for mere quibbles over definitions. (Pirie, 1937; Bowden and Pirie, 1937). Life implies organization, and a gene has vastly less organization than a yeast, or

even a bacterium. The only practically significant approach to an answer is to consider whether genes are at a high enough level of organization and action so that there is value in applying to them the concepts that typify the biological sciences.

We have found that genes are able to reconstitute suitable nutrient material into identity with their own substance. They are capable of reproduction, in the sense that when a gene has generated material to the extent of reduplicating its mass, it is present as two complete and separate genes. They have a certain degree of instability, whereby they undergo alteration into new types that can thereafter perpetuate themselves. In short they unite the characteristics that render an organism capable of metabolism and amenable to natural selection and to the biological type of evolutionary processes. Consequently our first answer to the question, "are genes living?" is that in certain respects their status in research science requires us to handle them on the basis of characteristically biological concepts. To just that extent, then, it is scientifically good sense to consider them living. But this consideration places no prejudice against studying their chemistry in terms of chemical concepts, or against accepting evidence for a simpler constitution than is ordinarily looked upon as compatible with the adjective "living", and the practical validity of this statement must remain unimpaired even although the exact chemical configuration of representative genes should become known.

SUMMARY OF CONCLUSIONS, PARTS I AND II

In rehearsing the factual observations and the theoretical considerations respecting heredity, we find that any scientifically mechanistic account of the processes must be in terms of extremely minute physicochemical units whose characters are to be learned by inference, at least to a considerable extent. For some of the inferences we can claim an almost conclusive cogency; for some others the most that can be said is that they give valuable suggestions respecting an uncertain condition of affairs. Keeping in mind, then, that much of the subject still lies on the outer skirmish line of scientific exploration, nevertheless the following propositions seem to be justified:

(1) The actual material and structure of the genes is unknown, but the matrix in which or on which they are located and from which they must derive their substance, is a combination of nucleic acid with special proteins characterized by nitrogen-rich amino acids.

(2) It is uncertain as yet whether each gene consists of a single, huge molecule, or whether it may consist of a limited cluster of molecules. Certain lines of inference point toward a single molecule.

(3) The genes constitute but a small fraction of the mass of the chromosomes in which they are located. If we consider only the crucial differential substance, they cannot much exceed 3 per cent of the material content of salmon sperm heads. In *Drosophila*, different lines of evidence indicate a percentage of from not over 0.4 per cent up to perhaps 4.4 per cent.

(4) In the types of animals we have considered, the genes are ultramicroscopic, their size doubtless never exceeding 20 $m\mu$ in diameter, corresponding to a maximum volume of 4190 cu.$m\mu$. It is unreasonable and superfluous, on the present evidence, to suppose that their principal dimensions fall below those of a molecule of ovalbumin, viz., to below linear dimensions of some 3.1×4.2 $m\mu$. On a globular basis this gives a minimum volume of perhaps 38 cu.$m\mu$., but if construed on a lamellar basis the minimum might be as

low as 9.5 cu. mμ. There are reasons for suspecting that these maximum and minimum bounds are well outside the actualities in the organisms that we have considered. The minimum volume is set by general considerations that apply wherever genes exist. The maximum is set by observations that might differ in other phyla of life (as for example the Liliaceae). This upper limit is not unthinkably small, as it exceeds the estimated minimum size of virus particles, and falls short of the highest estimates for the smaller viruses only to a reasonable degree.

(5) If the genes have a protein constitution (as is very probable), they may reasonably be pictured as having an internal lamellar structure, such as is shown by X-ray studies to be widespread among the proteins. During autosynthesis the hollow polyhedral form is hardly possible for genes, although it might be assumed at other times. Such lattice-shaped lamellar patterns as the hexagonal cyclol proposed for proteins by Wrinch (see figs. 3–7) or the rectilinear lattice described by Astbury and Woods, and by Clark and Shenk (Fig. 2), conform particularly well to the stipulations appropriate for a gene. An amino acid count lying between 70 and 80 to each lamella, corresponding to a protein molecular with 280 to 320 amino acids (or perhaps more specifically 288), is indicated for so long a list of native biological proteins, that one is impelled to bring up the possibility that the gene molecules and their lamellae carry the same numbers or perhaps some related characteristically stable number of amino acids. Their biologically reactive groupings may be incorporated in the lamellae, or may exist as attached prosthetic radicles.

(6) The chain of genes is carried in the chromonema, held in its alignment by chemical bonds arranged in a non-polar pattern. That is, either the gene molecules are basic proteins held in the chromonema by longitudinal nucleic acid molecules, or they are nucleoproteins some of whose nucleic acids straddle from gene to gene, or they are nucleoproteins alternating with a basic protein filling substance, to which they are bound on both sides by their nucleic acid valencies. In any case the chromonema structure depends at least in part upon acid bonds of nucleic acid, presented in both directions longitudinal to the chromonema, and is not to be explained by the hypothesis of longitudinal polypeptides that possess amino and carboxyl groups at their opposite ends.

(7) The total number of genes is moderately large, in *Drosophila* at least 2,000 for the haploid count, and possibly considerably more. No evidence now on hand calls for more than 5000, and they cannot be in excess of 13,000. There is some basis for the tentative surmise that the gene count in man may lie between 3.3 and 6 times that in *Drosophila*, viz. a haploid count between 6600 and 78,000.

(8) Among animals the genes of the haploid count are for the most part individually different from each other in a qualitative manner, and there is no wholesale duplication of comparable genes. A great part of their individual peculiarities and some, at least, of their mutations, are qualitative rather than quantitative.

(9) Genetic distinctions due to quantity differences (reduplication) are frequent and important in the higher plants. In animals quantity differences also exist, but seem to play a far less important rôle.

(10) Each gene has two types of action. The first is autocatalytic, whereby it conditions the formation of further molecules having the same peculiarities as its own. This power is highly specific, and yet flexible, in the sense that when altered by mutation the new gene perpetuates its new structure. It is also self-limiting in

the sense that the increase of mass is checked as soon as the mass is doubled and is not carried further till the chromosome has been divided. This specificity and self-limitation may well stand in some relationship to the probable laminated structure of their protein frame-work, or to some other unknown feature in their state of aggregation. It is possible that the pairing of like genes during maturation may be a further expression of the same insufficiently explained peculiarity that stabilizes their duplex condition. This chemical specificity is remarkably comparable to the specific selectivity of crystal formation.

(11) In addition to the autocatalytic effect they exert as their second activity an enzyme-like control over the formation of active substances that gives them a sort of long-distance control over cytoplasmic happenings. The active principles that they emit may well include hormone-like substances, but are doubtless in the main enzymes, possibly of the kinase type, or enzymes that generate other enzymes, perhaps some of them enzymes that generate hormones.

(12) As part of the evolutionary process, a gene must be credited with a liability for undergoing chemical alteration to produce a new gene substance with a slightly different molecular constitution, capable of autocatalysing itself, including the new item in its constitution. It would be theoretically conceivable that enzymes and hormones of the parent organism might sometimes induce heritable alterations in the structure of a gene, but experiments with transferred ovaries indicate that this does not actually occur. Most of the observed examples of heritable mutations reveal themselves as random losses or weakening of gene potency, but some cases of augmentation have also been verified. It has been found possible in a few instances to gather evidence that there is a qualitative aspect in the change of physiological effect, even where the effect produced is of a quantitative nature. These mutations occur in a sense at random, yet in accordance with the laws and limitations set up by the physico-chemical constitution of each mutating gene.

(13) Quantitative changes in the gene equipment also occur by "mutation." The best authenticated examples of these are numerical changes such as reduplication. Quantitative impoverishment of a gene could be supposed, theoretically, if the gene contains more than one molecule, but there is usually no way to differentiate between quantitative impoverishment and qualitative impairment of a catalytic agent; hence the great majority of mutations remain in an undistinguished class that may be either qualitative or quantitative.

(14) The two processes of gene reduplication and single-gene mutation, taken together, provide a possible mechanism by which a complex genetic machinery may be evolved out of simpler antecedents. Such a result might come to pass either through reduplication of genes followed by mutation alterations, or by a qualitative mutation followed by the intrusion of the altered gene into a chromosome beside the old gene.

(15) In the evolution of organisms, genes play a part, not merely by supplying the mutations among which natural selection must select, but furthermore by setting up barriers of mutual sterility, possibly through various kinds of translocations (producing semi-sterilities, etc.) or through serological mutations, or by releasing conditional lethals or making non-viable combinations. Only through the establishment of such physiological isolation is it possible for the path of evolution to bifurcate and produce radical

diversities. The extreme preciseness of the serial arrangement of genes in the normal germ cell finds much of its significance in its bearing on the mutual compatibilities and incompatibilities of biological strains. In other respects the space relations of the genes carried in a nucleus have at most only a small effect on heredity.

(16) Although small enough to be in the class of colloid micels, and probably not too complex to permit of the ultimate hope of learning much about their molecular constitution, we must still view the genes from the evolutionary standpoint as essentially living units, because they are subject to the biological type of evolutionary process. Very possibly they may be the smallest ultimate units that function according to biological categories.

(17) The ontological development whereby offspring come into the characteristics predetermined by their genic constitution is essentially a chapter in physiology. The evidence points in the direction that different organs in one organism are not customarily provided with very great (if any) differences in gene content, but rather that the differentiation comes about through a progressive physiological alteration of the cellular medium in which the genes are working, through accumulated chemodifferentiation, reciprocal hormonal effects, altered metabolic gradients, and the like. Thus every effect said to be produced by a gene is really brought about by reciprocal action of a gene with the rest of the cell, and in spite of the particularistic machinery the outcome always carries a totalitarian complexion.

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SECRETIONS FROM ECTODERMAL GLANDS OF ARTHROPODS

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THE products of secretion of the different types of hypodermal glands differ greatly and show that cells of a common embryonic external layer (the ectoderm) eventually come to produce substances of, often, entirely unrelated composition. The cuticle in general and the cuticular pigments are hypodermal secretions which, however, will not be treated here. There are many secretions of hypodermal origin, the physiological or chemical properties of which have never been studied and which cannot therefore be included in this review.

A. PRODUCTS OF THE "TEGUMENTAL" GLANDS

When, prior to the molting of a decapod crustacean (Yonge, '32), the newly forming cuticle beneath the old cuticle is about 2 microns thick there is, here and there, a secretion apparent at the outer surface of the new cuticle at openings of the ducts of the tegumental glands (Fig. 1, *t*). The new epicuticula is not as yet formed. After each molt these glands degenerate and eventually disappear. New glands develop between molts and display the greatest secretory activity at the time when the epicuticle, *ep*, is being most rapidly formed. There are very distinct differences in physico-chemical constitution between the epicuticle and the rest of the cuticle. Yonge further showed that the properties of the epicuticle are the same as those of the fluid contained in the ducts, *dt*, of the tegumental glands. Thus, both the epicuticle and the fluid in the ducts have a special affinity for basic stains (therefore, have an acid reaction), an isoelectric point at pH 5.1, are resistant to concentrated mineral acids, are not attacked by chitinase, and yield a positive test for lipoids. The conclusion that the epicuticle of decapod Crustacea is the product of the tegumental glands appears evident.

The secretion of these glands, apparently a lipoid material, must possess a low surface tension to be capable of spreading as a thin sheet over the surface of the cuticle. During the process of its formation the new epicuticle is protected by the old cuticle. By virtue of its very low surface tension, the secretion of the tegumental glands surrounds the otoliths of the statocysts and thus connects them, by thin strands, to the setae (Prentiss, '01; Lang and Yonge, '35; Yonge, '35a). In the same manner, the eggs are attached to the pleopods of the female (Yonge, '35b, '37, '37a). "The intimate association between ovulation and secretion by the oviduct and the cement (tegumental) glands points, in the apparent absence of nerve connections to the cement glands and also in the oviducal epithelium, to the presence of some controlling hormone."

Such glands in decapods have been

described anatomically by Vitzou (1882), Herrick (1895), and, according to Yonge, by Braun and Farkas. They occur over the entire surface of the body in all decapods studied.

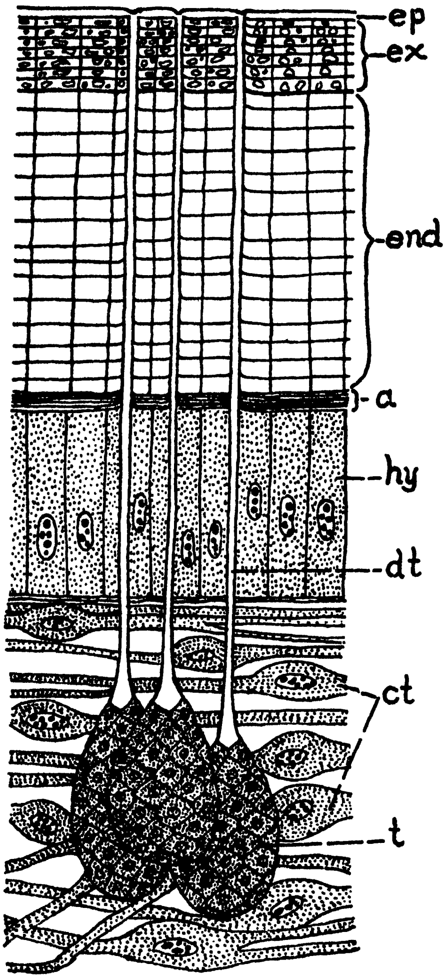

FIG. 1. GENERALISED TRANSVERSE SECTION THROUGH THE FULLY FORMED INTEGUMENT OF A DECAPOD

a, accessory cuticular layer; *ct*, conjunctival tissue; *dt*, duct of tegumental gland; *end*, endocuticula; *hy*, hypodermis; *t*, tegumental gland. (Adapted from Vitzou; Verne; and Yonge.)

In insects, on the other hand, the epicuticle is the first layer of the cuticle to be formed and is secreted by the whole outer surface of the hypodermal cells (Wigglesworth, '33).

It may be that the epicuticle of arthropods protects the newly forming cuticle beneath from the digestive action of the exuvial glands. The latter secrete a fluid which digests a considerable amount of the organic material of the old cuticle (see below).

B. MUCOUS GLANDS

Certain glands in the gills of brachyuran and macruran Crustacea (Cuénot, 1895) secrete a mucous-like substance which gives the typical colors of mucous with various basic dyes. In some cases, when a crayfish (*Cambarus bartoni*) is allowed to reduce the oxygen concentration of the external medium it secretes a protein, probably a glucoprotein such as mucous, when the oxygen concentration falls to a low level. This protein acts as a buffer by combining with some of the excess carbon dioxide in the medium (Maloeuf, '36).

It is not surprising to know that modified hypodermal cells secrete mucous, for, the hypodermal cells secrete chitin, which, like mucous, yield a monosaccharidamine upon hydrolysis. This amine is glucosamine in chitin, and galactoseamine in mucous.

The secretion of mucous by glands in the gills of certain Crustacea results in a coating of the gills when the animals are out of the water and thus prevents dessication through the gill filaments and, also, probably aids in gaseous exchange by moistening the cuticular surface of the gill filaments.

In the crustacean *Chirocephalus diaphanus* (Cannon, '28 and '35) the food particles are arrested in the maxillary region by the entangling secretion of the labral glands, and the mass is pushed into the mouth by the maxillules.

C. LIPIDS

1. Oil. Oils and fats are fatty acid esters of the triatomic alcohol glycerin. "Oil" here is synonymous with a liquid fat and is, thus, non-volatile. Unicellular glands, which probably secrete the oil which renders the spiracular tubes of mosquito larvae hydrofugous, have been observed by Keilin, Tate, and Vincent ('35) and their predecessors.

2. Fat. The "woolly louse," *Schizoneura lanigera* (Aphididae), is so called because of the presence on its cuticle of "waxy" or "wooly" fibers. These are the products of numerous hypodermal glands. Schulz ('22) has shown that these fibers are not wax at all, as they grossly appear to be, but are a glycerid of a saturated fatty acid or of saturated fatty acids and are, therefore, fat. The kind (or kinds) of fatty acid entering into the constitution of the fat has (or have) not as yet been determined.

Fat-secreting glands occur at the posterior end of the abdomen of bees and wasps. They lubricate the sting stylets (Heselhaus, '22).

3. "Wax." "Waxes" are generally mixtures of fatty acid esters of mono- or dihydroxy saturated alcohols and paraffins. At one extreme, cocksfoot wax consists almost entirely of primary alcohol while, at the other extreme, tobacco leaf wax is made up exclusively of paraffins. "Wax, in a broad technical sense, is a hard, high-melting material containing no glycerides, and not in the strict chemical sense of a long chain ester" (Chibnall and Piper, '34).

Wax is secreted by the aphid "wooly louse" *Pemphigus xylostei* (Schulz and Becker, '31) during all stages subsequent to the first larval instar, by the beetles *Lindorus lophanthae* and *Rhizobius ventralis* (Flanders, '30), and by bees. Wax glands are unicellular and the cuticle bounding such is very thin. Rogojanu ('34) could find no pores in the coccid, *Eriosoma*, and thus concluded that the wax makes its exit in a liquid state. In the case of the coccid, *Orthezia*, he observed wax to be discharged at the apex of a hollow hair by a rupture of the tip. In the case of the honeybee, Dreyling ('03) stated that in order to observe pores to the exterior it is necessary to make very thin sections and observe through highest magnifications. Wax pores lined with cuticle have also been described in dragonflies (Schulze, '34) and coccids (Pollister, '37).

Until the end of the eighteenth century it was commonly supposed that the wax of bees is manufactured directly out of pollen ("farina"). That wax and pollen are two distinctly different substances was shown to be so by Hunter (1792) who arrayed the following crude yet substantial facts in support of his demonstration:

1. Pollen burns, but in so doing does not smell like wax. 2. Pollen is of different colors on different honeybees, whereas newly made comb is all of the same color. 3. Pollen is gathered with greater avidity for old hives in which the wax combs are complete. 4. In the first formation of the combs of a hive the bees seldom bring in any pollen. Pollen is later brought in for the hatched larvae. 5. As much comb is formed when the swarm is in the hive, i.e. during cold or wet weather, and when the bee thus cannot secure pollen. 6. Wax is *secreted* externally between the scales of the underside of the abdomen. 7. "I took several scales on the point of the needle and held them to a candle, whereas they melted, and immediately formed themselves into a round globe." 8. These scales are found only during the time when wax combs are being constructed.

Hunter's observations are convincing even though they do not quite attain up-to-date analytical requirements. While the wax of bees is the product of the synthetic activity of the modified hypodermal cells of the abdominal sterna, the raw materials out of which wax is

synthesized come from the honey ingested—a carbohydrate food, and not from pollen—mainly a protein food. Chibnall and Piper ('34) have suggested a probable scheme for the metabolic formation of waxes from sugars.

A study of the chemical constitution of the wax secreted by *Pemphigus* was made by Schulz and Becker ('31).

They ascribed the following structural formula to this wax:

$$\begin{array}{l} CH_3\cdot(CH_2)_{15}\cdot CH(OH)\cdot(CH_2)_{16}\text{—}CH_2 \\ \qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\quad \searrow \\ \qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad O \\ \qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\quad \swarrow \\ CH_3\cdot(CH_2)_{15}\cdot CH(OH)\cdot(CH_2)_{16}\text{—}CO \end{array}$$

or the empirical formula, $C_{68}H_{36}O_4$. It was considered that this wax could be split up into the diatomic alcohol, "pemphigus alcohol" [$C_{34}H_{68}(OH)_2$] and the fatty acid, "pemphigus acid" ($C_{33}H_{67}$-$OH\cdot COOH$). This wax, they stated, is not identical with that secreted by the psyllid bug, *Psylla alni*, but is closely similar to that of the bettle *Coccinella*.

The important work of Chibnall and his school (Chibnall, Latner, Williams, and Ayre, '34; Piper, Chibnall, and Williams, '34; Chibnall, Piper, Pollard, Williams, and Sahai, '34; Chibnall and Piper, '34; Pollard, Chibnall, and Piper, '31; Francis, Piper, and Malkin, '30) with the use of X-ray analysis, has shown, however, "that all the names which have been assigned by previous workers to primary alcohols, fatty acids and paraffins isolated from waxes have been given to products that are not chemical entities but mixtures." Among others, this refers to Liebermann's (1885, 1887) work on the wax of *Coccus cacti*, Gascard and Damoy's ('23, '23a) studies on bees' wax, Sundwick's ('01) work on *Psylla* wax and the paper of Schulz and Becker (*loc. cit.*). Chibnall *et al.* showed that cochineal wax from *Coccus cacti* is relatively simple, consisting of 15-keto-*n*-tetratriacontanol (cocceryl alcohol), *n*-triacontanoic acid, and 13-keto-*n*-dotriacontanoic acid. Bees' wax was much more complex containing all the even-number primary alcohols from C_{24}–C_{34} inclusive and probably all the even-number *n*-fatty acids from C_{24}–C_{34} in addition to four paraffins. Chibnall *et al.* showed that all the primary alcohols and *n*-fatty acids present in waxes contain an even number of carbon atoms and that all waxes are essentially of the same general type; have even-number primary alcohols and *n*-fatty acids from C_{24}–C_{36} and odd-number paraffins from C_{25}–C_{37}.

The yellowish color of wax secreted by honeybee workers is due to the presence of a flavone derivative, possibly 1.3-dioxyflavone (Jaubert, '27).

The behavior of bees when in the process of wax and comb manufacture is very interesting and has been described by Comstock (1895).

"When a colony needs wax", wrote Comstock, "many of the workers gorge themselves with honey, and then hang quietly in a curtain-like mass . . . After about twenty-four hours there appears on the lower surface of the abdomen of each bee little plates of wax that are forced out from openings between the ventral abdominal segments called wax-pockets. Other workers attend to this curtain and collect the wax as fast as it appears, and use it at once in constructing combs. As it requires about twenty-one pounds of honey to make one pound of wax, the bees, who are true economists, make this expensive product go as far as possible. The hexagonal cells of the honeycomb afford an ideally compact arrangement, as if the bees were mathematicians and had fully planned to secure the most room in the least space. The cells of the comb are used both for rearing the young and for storing food. . . . Honey is regurgitated into the cells of the comb."

The honey stored by the bee is digested nectar which is transported to the hive and stored as an easily assimilable source of carbohydrate for the synthesis of wax. Thus, by supplying the bee hive with artificial combs, the bee-keeper makes it possible to breed bees for profit by selling stored honey.

In part because of the use of wax in churches, bees have been upheld as sacred animals. "The wax of bees," wrote de Gubernatis (1872), "because it produces light, and is, moreover, used in churches, must have had its part in increasing the divine prestige of bees." The "virgin birth" (parthenogenetic development) of the drones has also contributed to the sanctity of bees. Thus Aristoteles wrote that the hornets and wasps "have nothing divine about them as the bees have. For the so-called 'mothers' generate the young . . . but they generate by copulation with one another, for their union has often been observed." Parthenogenesis, nevertheless, occurs in wasps too.

D. PRODUCTS OF THE EXUVIAL GLANDS

1. Occurrence and structure of the glands. Exuvial (molting, or Versonian) glands, were probably first definitely observed by Verson (1890) in silkworm larvae, in each of which there are fifteen pairs of such glands. Each one of these glands, in the silkworm, is a very much enlarged and vacuolated hypodermal cell communicating with the outer surface of the old cuticle by a relatively wide and short duct (Verson, 1890; Wachter, '30). In the larvae of the bug, *Rhodnius* (Wigglesworth, '33), each gland is formed of three hypodermal cells and communicates with the exterior by means of a fine duct which is apparently lined with cuticle and extends into the interior of the gland. The glands are absent from the adult bugs.

The cells, or cell, of these glands enlarge and become vacuolated prior to the period of molting (ecdysis). Tower ('06) observed that after ecdysis and especially after pupation the exuvial glands of coleopterous larvae degenerate rapidly. The studies of Hoop ('33) on the exuvial glands have shown that, in the dipterous larva, *Limnophila fuscipennis*, very large cells lie in the haemocoele against the much smaller hypodermal cells. Only the last larval molt was investigated in this case, i.e. that prior to the formation of the pupa. Very shortly before the molt the cells enlarge and become vacuolated. After the molt they shrink and lose their vacuoles. The same applies to the hymenopterous larva, *Nematus*. These large monocellular glands, unlike the exuvial glands of *Rhodnius*, do not degenerate even after the final molt.

Other somewhat smaller cells which contain large globules just before a molt but which degenerate after the last larval molt (this being the only molt investigated) were observed in the hypodermis of *Limnophila*.

In no case did Hoop find a duct leading to the outer surface of the cuticle in any insect studied. No special exuvial cells could be found in larvae of the blowfly, *Calliphora*, or in the tracheae or fore and hind guts of any species. It should be noted, nevertheless, that the presence of molting fluid between the old and new cuticles in all the above cases as well as in between the old and new tracheal cuticles, indicates that some sort of fluid-secreting (exuvial) glands are present in all cases. Hoop, therefore, suggested that, in *Calliphora*, the hypodermal cells as a whole secrete the exuvial or molting fluid.

It may be significant to note that Wigglesworth found that the oenocytes (relatively large ectodermal cells confined to the hypodermis in *Rhodnius*) enlarge prior to a molt and later become reduced in size. They are, however, probably not directly concerned with the secretion of the exuvial fluid since they are quite ductless and persist in the adult. Suggestions as to the functions of the oenocytes are offered below.

Exuvial glands, so far as I know, have not as yet been described in other arthro-

pods. They probably will be found, however, if looked for.

2. Function. It is a generally observed fact that, at the time when the old cuticle is cast off, insects are almost dry. Shortly prior to the act of molting the silkworm (Wachter, '30) swallows its molting fluid. This is followed by a wrinkling of the old cuticle. In the bug, *Rhodnius* (Wigglesworth), the molting fluid is not swallowed but is reabsorbed through the new cuticle. It is therefore quite evident, as Wigglesworth pointed out, that, *whatever the function of the molting fluid, that function is exerted before the act of molting,* for, the molting fluid is then abundant between the old and new cuticles.

What then is the function of the molting fluid? Wigglesworth has found that fully 86.5 per cent of the old abdominal cuticle of *Rhodnius* is reabsorbed before molting. The decrease in depth of the old cuticle as time for ecdysis draws near is very marked. The marked loss in mass of the shed cuticle has been observed in insects and other arthropods and has been quantitatively studied by Herrick (1895), Drach ('35 a, b), and Robertson ('37) in decapod Crustacea. Since the cuticle contains protein and chitin, Wigglesworth concluded that the exuvial fluid must contain a proteinase and a chitinase. The presence or absence of enzymes was not, however, determined; nor were the absolute amounts of chitin in the exuvium (cast-off cuticle) and in the old cuticle, before its shedding, analyzed. While, therefore, it seems probable that the chitin of the old cuticle is partly digested and absorbed, we are not sure that this is the case. Chitinase has not as yet been definitely found in arthropods and the discovery of its production by the exuvial glands would be of interest.

"There is no doubt, I believe, that you would find less chitin in the exuvium than in the fully developed cuticle. It seems to me that there must be a chitinase in the molting fluid to break down the endocuticula of the old cuticle and to build it up into the new" (Campbell, personal communication).

In view of the fact that enzymes possess protein properties and that proteins cannot diffuse through cell membranes, the discovery of Hoop and Wigglesworth that the exuvial glands disintegrate after each molt acquires meaning. The cell membrane must rupture at some point to liberate the enzymes. It should be mentioned, however, that Hoop did not recognize the significance of this for he wrote that "these latter are considered molting glands by some but surely cannot be so owing to their subsequent degeneration."

By the method of injections into various moth larvae, Schürfeld ('35) has indicated that the exuvial glands are concerned solely with the liberation of the exuvial fluid and do not secrete a molting or a metamorphosing hormone.

E. PRODUCTS OF THE LABIAL GLANDS

These glands arise in the embryo as paired ectodermal invaginations of the ventral region of the second maxillary (labial) sternum of insects. They are often known as "salivary glands." As will become apparent below, they do not in all cases contain digestive enzymes, as do the salivary glands of vertebrates, and often serve other functions, such as preventing the coagulation of the blood of vertebrates. In this category also belong the silk glands of hymenopterous and lepidopterous larvae. In arachnids, "myriapods," and *Peripatus*, what are possibly homologues of these glands open into the anterior end of the fore gut. There is as yet no satisfactory evidence for the presence of such glands in the Crustacea.

1. "*Saliva*"

(*a*) *Hydrolyzing enzymes.* The macerated salivary glands of the scorpion (Sarin, '22), a carnivorous animal, contain no catalase, amylase, inulase, and no sucrase. They do contain the proteolytic enzymes, pepsinase and trypsinase. Lipase and chymosinase (a milk coagulant) were noted in one out of the three cases studied. The secretion of proteolytic enzymes from the salivary glands of scorpions and spiders is typical of the condition among arachnids since all living arachnids except *Limulus* (Yonge, '37) have adopted a suctorial habit which may be partly responsible for the retention of some degree of intracellular digestion in these animals.

The salivary glands of honeybee larvae do not secrete an amylase even though the blood contains this enzyme (Bertholf, '27). Accordingly, these larvae were found incapable of utilizing starch or glycogen. The demonstration of the secretion of sucrase and amylase by the salivary glands of honeybee worker adults, on the other hand, appears conclusive. Pavlovsky and Zarin ('22) offered a thick aqueous, enzyme-free solution of sucrose to honeybee adult workers. The honey which the bees subsequently regurgitated contained sucrase and amylase. They do not consider that these enzymes could have been introduced into the "honey stomach," or crop, from the mid gut for two reasons. Firstly, the valves between the "honey stomach" and the mid gut would probably prevent an exgurgitation of the mid gut contents; and, secondly, catalase, which is invariably present in the mid gut, would appear in the "honey stomach" if exgurgitation from the mid gut occurs. Furthermore, whole extracts of the "honey stomach," containing no honey, never revealed the presence of any such enzymes. They therefore concluded that the sucrase and amylase found in the exgurgitated honey are produced by the salivary glands. Küstenmacher ('11) also found honey to possess some amylolytic action and thus concluded that the salivary glands secrete amylase since this enzyme is not present in the pollen and nectar eaten by the bees. In contrast to Pavlovsky and Zarin, Küstenmacher did not consider it necessary to assume that the salivary glands secrete sucrase since this enzyme is found in the pollen eaten. While Pavlovsky and Zarin apparently banished Küstenmacher's objection that the sucrase may have entered the "honey stomach" from the mid gut, they also swept out of existence his suggestion that the sucrase may have come from the pollen by feeding bees on an enzyme-free solution of sucrose. It still remains to be discovered, however, why honeybee adults, the salivary glands of which apparently secrete amylase, cannot, according to Phillips ('27), utilize starch or glycogen for food.

When sucrose, a disaccharid, is acted upon by sucrase it is hydrolyzed into its two component monosaccharids, l-fructose and d-glucose, and becomes "honey," or, in technical terms, invert sugar. Sucrose is dextrorotatory (i.e. rotates the plane of polarized light to the right). Invert sugar is laevorotatory (i.e. rotates the plane of polarized light to the left). This is because, though the glucose component of invert sugar is dextrorotatory, the fructose constituent is more laevorotatory than the glucose is dextrorotatory so that the algebraical effect of a solution of glucose and fructose in equal concentrations is to turn the plane of polarized light to the left instead of to the right as the mother solution, sucrose, did—hence the

term "invert sugar." The salivary glands of the bee secrete sucrase and amylase. These enzymes hydrolyze the sucrose and starch present in the nectar and which is stored within the crop, or "honey stomach." Sucrase hydrolyzes the sucrose into glucose and fructose while the amylase hydrolyzes the starch, that may be present, into maltose. The latter is split by maltase into glucose. Prior to 1917, many practical bee-keepers added small amounts of citric acid to the winter food of bees with the idea that the production of honey is thereby augmented. Zarin ('17) showed the practical value of systematic scientific research when he demonstrated that 0.1 per cent citric acid produces no increase in the rate of inversion of cane sugar and that 0.3 per cent citric acid definitely inhibits such action.

Since the mandibular glands of the honeybee begin to discharge their secretion two days prior to emergence from the pupal case, Dreher ('36) suggested that they serve to soften the cocoon.

The salivary glands (?) of the stick insect, *Dixippus morosus* (Belehrádek, '22), judging from the digestive juice which issues from the mouth and which is considered to contain products of salivary glands, secrete cellulase and a powerful amylase which is most active in alkalin solution. Reducing sugar is formed but only in alkalin solution. This juice has no action on fats and proteins.

The salivary extract of various species of the onychophoran *Peripatopsis* (Heatley '36), contains enzymes (amylase, glycogenase, protease) which act on large molecules, reducing the food to a semifluid state. This is then acted upon by the gut enzymes. The saliva of certain Coccidae (*Aspidiotus*, *Dactylopius*) and Capsidae (Smith, '26) appears to be capable of dissolving the cell walls of the plants through which the stylets penetrate and hence evidently contains a cellulase. The saliva issuing through the stylets of certain other Hemiptera, on the other hand, gave no evidence of such action but apparently contains a protease since the plant cell contents were destroyed over a wide area.

The enzymatic activities of the salivary glands of the cockroach (*Periplaneta orientalis*) and of several species of moth larvae (*Phalera bucephala*, *Lymantria dispar*, *Macrothylacia rubi*, and *Cerura vinula*) were studied by Dirks ('22). In all cases the glands exhibited strong amylolytic and sucrolytic properties; but there was no maltase, lactase, cellulase, glycosidase, lipase, or protease. It was suggested that the amylase and sucrase are synthesized by the salivary glands themselves and are not absorbed from the blood, for, the salivary glands were not found to possess the maltase and protease present in the blood. Furthermore, it is extremely unlikely that enzymes, as such, can diffuse into membranes. It is noteworthy that, as the above results show, the salivary glands of lepidopterous larvae which secrete silk also contain, and possibly secrete, hydrolytic enzymes. The salivary secretion of the cockroach, *Blatella germanica*, has a pH of about 6.9 (Wigglesworth, '27). This indirectly corroborates the work of Dirks since the action of most amylases is most rapid in slightly acid media. Basch (1858) was the first to show that the saliva of a cockroach (*Blatta orientalis*) has amylolytic and proteolytic activity.

The salivary glands of the adult blowfly, *Calliphora* (a non-blood-sucking fly), secrete an amylase in definitely pronounced quantity. The salivary secretion of blood-sucking insects such as tsetse fly (*Glossina*) adults and tabanid fly (*Chrysops silacea*) adults, on the other hand, do not secrete amylase nor, in fact, any hydrolyzing enzyme. The salivary glands of

Calliphora do not secrete sucrase, maltase, lactase, trypsinase, pepsinase, or peptidase, while the secretion of a lipase remains questionable (Wigglesworth, '29 and '31). Swingle ('28) could not find any hydrolyzing enzymes in the salivary glands of larval and adult oriental fruit moths (*Laspeyresia molesta*) and Roy ('37) noted that extracts of the salivary and mandibular glands of larvae of the moth, *Galleria mellonella*, show no hydrolytic action.

(*b*) *Anticoagulins*. In 1899, Sabbatani discovered that aqueous extracts of whole ticks, *Ixodes ricinus* (ectoparasitic on dogs) render the blood of vertebrates incoagulable. Both small and large ticks were potent in this respect. Blood from a dog into which a strong dose of such an extract was injected could render blood from another dog incoagulable. The production of an antibody shows that the anticoagulin is a protein. Equal dosages were found to have effects on the blood of different mammals in the following descending order: dog>cat>cow>sheep. Tick extract was also found to have similar effects on the blood of pigs and of frogs. Probably owing to the rapidity of coagulation of the blood of the poultry cock and of the pigeon, results obtained on these were negative. Sabbatani concluded that the substance preventing coagulation is an enzyme (protein), for its activity was destroyed by boiling. It was considered to be an antienzyme acting against the "filbrino-ferment" present in the blood of vertebrates. The salivary glands and gut of the tick, *Argas persicus* (Nuttall, '08), contain an anticoagulin which is inactivated by a ten-minute exposure to a temperature of 80°C. No organs in this tick were found to contain haemolysins. But Pawlowsky and Chodukin ('29) have found that the salivary glands of a tick, *Ornithodorus papillipes*, contain a substance which is capable of haemolyzing human erythrocytes.

Several papers have since appeared notifying the presence of anticoagulins in the salivary secretion of various ticks and blood-sucking flies. Thus Cornwall and Patton ('14) found the anticoagulin activity of *Musca crassirostris* to be powerful; *M. pattoni*, week; *Tabanus albimedus*, powerful; *Anopheles persicus* (mosquito), powerful; *A. fuliginosus*, powerful; and *Argas persicus* (tick), fairly powerful. They found no anticoagulin in the salivary secretion of *M. nebulo*, *M. convexifrons*, *Stomoxys* sp., *S. calcitrans* (flies) and *Cimex rotundatus* (a flea).

A thorough and noteworthy paper on the subject is that of Lester and Lloyd ('28) on the anticoagulin of the salivary secretion of the adult tsetse fly, *Glossina*. The powerful anticoagulin secreted by the salivary glands of *Glossina* delays the clotting of the blood of mammals, birds, reptiles, and amphibians. If the salivary glands are removed the fly can still draw blood normally and live for some time. After such an operation, however, large blood clots begin to form in the narrow anterior portion of the gut. The fly eventually dies merely of starvation. This shows that the gut does not contain an anticoagulin and indicates that the anticoagulin found in the intestine of *Argas* probably arrived there from the salivary glands. Lester and Lloyd noted that the hinder part of the mid gut of *Glossina* contains a coagulin which causes rapid clotting of blood in that region and thereby causes a retention of the blood in the fluid state in the mid gut thus allowing hydrolysis of the blood constituents. Both coagulin and anticoagulin have the properties of enzymes (proteins) in that they are destroyed by heat; the coagulin, being the more thermostable, is rendered inactive at 90°C. They are soluble in water, dilute salt solutions and dilute ethyl alcohol, but are quite insoluble in ether and absolute alcohol and are pre-

cipitated by half saturation with ammonium sulfate.

The mechanism of blood coagulation in vertebrates is commonly conceived to proceed thus:

$$\text{(a) Thrombokinase (enzyme)} + \text{thrombogen} \xrightarrow{\text{(Ca salts)}} \text{thrombin}$$
$$\text{(b) Thrombin} + \text{fibrinogen} \rightarrow \text{fibrin} \downarrow$$

At which points in the above reactions does the anticoagulin act? Lester and Lloyd found that the coagulin in the mid gut will not coagulate decalciated blood. They therefore suggested that the salivary anticoagulin is an antithrombokinase (antikinase) and that the coagulin in the hind intestine is akin to thrombokinase (kinase) but is not inactivated by the salivary anticoagulin.

Certain blood-sucking insects, e.g. *Stegomyia fasciata*, *Anopheles bifurcatus* (Yorke and MacFie, '24) and the tick fly *Hippobosca* (Wigglesworth, '30, personal communication from L. Lloyd), have no anticoagulins. At least in *Hippobosca*, an anticoagulin is unnecessary since the animal "spends nearly all its life upon the back of its host, sipping a little blood whenever it desires." Furthermore, it has no crop and "clotting in its proboscis" must be prevented simply by cleansing it with saliva after the meal (Wigglesworth, '30).

The adaptive significance of the presence of erythrocyte agglutinins in the salivary secretion of various Diptera (Yorke and MacFie) is as yet unascertained.

2. "*Silk*"

"Silk" is any fine, glossy, fibrous substance. Thus, a type of artificial silk is produced by allowing a solution of celloidin to pass through fine pores. Under this topic only silk produced by labial glands will be treated. This includes the silk produced by hymenopterous, lepidopterous and some coleopterous larvae and also by certain dipterous larvae such as *Simulium* and *Micetophila*. In all these cases the silk is used in the manufacture of a cocoon for pupation. Spiders and ant-lions produce silk from glands situated in other parts of their bodies. Of the silk produced by *labial* glands only that manufactured by the silkworm has been subjected to rigorous physico-chemical analyses. It is hence impossible to say whether the silk produced by the various species is similar. It is a matter of common observation, nevertheless, that even the silk produced by different species of moths is not identical in texture or color.

(*a*) *Physical properties of silkworm* (*Bombyx mori*) *silk*. The histology of the silk glands of this domesticated insect is the main subject of noteworthy papers by Helm (1876), Gilson (1890), and Lesperon ('37). These glands disappear during metamorphosis (Malpighi, 1669). Silk, on leaving the spineret of a larva, is soft but rapidly hardens. It consists of two concentric cores of "fibroin" cemented together and covered with a "gum." It is the "fibroin" which gives the finished or "soft silk" of manufacture its pearly white luster. If the natural gum is left on during commercial manufacture the silk is termed "hard silk" by sericulturists. The gum is usually removed by mechanical action and soaking in warm water. Silk without gum is known as "soft silk." The maximum diameter range of silk fibers is 0.0009–0.0023 cm. A single fiber has the great tensile strength (stress to produce rupture) of 3 to 5 $\times$ 10^9 dynes/sq. cm., extending considerably before breaking. Hence the durability of silk clothes. Under limited tensions silk will revert to its original length and, therefore, possesses some elasticity. The specific gravity of a fiber is 1.3 and is thus somewhat heavier than water. Silk has a low thermal and very low electrical conductivity and is hence used as an insulator in human economy.

(*b*) *Chemical properties*. The product obtained after treating silk with dilute

alkali or hot water was called "fibroin" by von Mulder, in 1836, and "sericin" by von Cramer, in 1865. The amino acid, serin, was first obtained by von Cramer from the hydrolysate of silk fibroin, in which serin is present in large amounts. Hence the name of this amino acid (L. *sericum* silk). Von Mulder's indefinite term has been retained since "soft silk" is a more complex material than serin, and because "sericin" is liable to be confused with serin. Furthermore, "sericin" is sometimes synonymized with the "gum." Fischer and Skita ('01) and Abderhalden and Behrend ('09) made quantitative studies on the monoamino acids yielded upon hydrolysis of silk "fibroin" produced by the larvae of *Bombyx mori*. The former workers analyzed silk secreted by Italian larvae, and the latter, that secreted by Chinese larvae. Their values are close and are expressed in percentages of the dry weight of the "fibroin" (Table 1). Demianowski ('28) has noted the presence of the diamino heterocyclic acid, tryptophane, in silk. This biologically valuable amino acid is not known to be synthesized by animals (for a possible exception see Jezewska, '26) and Demianowski has found more tryptophane in the silk of weak or "unhealthy" larvae than in that of sound individuals. Apparently, adequate regulation of the secretion of tryptophane is not present in weak larvae. The fibroin contains 0.32 per cent ash in terms of dry weight and 5.1 per cent water (Abderhalden and Behrend). The *Encyclopaedia Britannica* ('32) on the other hand, states that, under ordinary atmospheric conditions, fibroin contains 11 per cent water. The empty cocoon of a silkmoth, *Bombyx mori* (Kellner, 1884) contains 12.5 per cent water. But the water content of silk soon after and during its secretion is very probably higher than that.

TABLE 1

Analysis of monoamino acids secreted by Italian and Chinese larvae of Bombyx mori

MONOAMINO ACID		ITALIAN LARVAE FISCHER AND SKITA	CHINESE LARVAE ABDERHALDEN AND BEHREND
Neutral aliphatic amino acid	Glycin	36.0	37.5
	d-Alanin	21.0	23.5
	l-Serin	1.6	1.5
	l-Leucin	1 to 1.5	1.5
	l-Phenylalanin	1 to 1.5	1.6
	l-Tyrosin[1]	10.0	9.8
Heterocyclic amino acid	l-Prolin	Present	1.0
Acid amino acid	l-Aspartic	Present	0.75
	l-Glutamic	0.0	—

[1] "In view of the high tyrosine content of silk it was of interest to examine quantitatively the tyrosine intake of the silkworm and compare it with that of the silk produced. The tyrosine intake in mulberry leaves ingested by the silkworms (*Bombyx mori*) was found to exceed the output of tyrosine in silk and contained groups; hence, there is no need to postulate a mechanism for the synthesis of tyrosine." (Holtzmann '36.)

X-ray examination shows that fibroin is crystalline. This, of course, would be expected. Fibroin is soluble in cold concentrated mineral acids and in concentrated KOH or NaOH from which it may be reprecipitated by neutralization. It may thus be a heterogeneous albuminoid protein.

The gum, or gelatin, is, in contrast to the fibroin, extremely low in glycin content but higher in serin content (5.4 to 6.6 per cent serin of dry weight). "The analyses of silk gelatin account for only 20 to 40 per cent of the amino acids which are present" in it (Gortner, '29). Fischer and Skita ('01) have made the statement that silk gelatin is, in contrast to the fibroin, rich in diamino acids. The "gelatin," or "gum," ranges between 15 and 20 per cent, by weight, of the raw silk (Gortner).

(*c*) *The formation of silk*. Machida ('27) has attempted to localize the region in the salivary glands of *Bombyx mori* which secretes the fibroin and that which secretes the gum (or "sericin," as he terms the latter). Portions of the salivary glands of third and fourth larval instars were dissected out. Stained and sectioned preparations were made of the salivary glands of operated mature larvae and adults. When acid fuchsin and methyl green were used the fibroin was stained blue and the gum purple. The quantity of fibroin in the middle division of the salivary glands was thus found to vary directly with the size of the attached posterior division. When the posterior division was separated from the middle division the latter division lacked gum but contained fibroin. The experiments indicate that gum is secreted throughout the whole length of the middle division and fibroin in the posterior division only.

"The silk is not actually pushed out of the gland by the activity of the gland itself. It is removed by the silkworm who, having fixed the thread to some object, moves away and so pulls out the thread of silk."

Does the silkmoth larva, before spinning, have all the monoamino acids contained in the fibroin or do the silk fibers undergo later changes? An attempt at answering this question was made by Abderhalden and Dean ('09). It is desirable, in producing the required demonstration, to remove and analyze only the silk glands since, while the whole body may possess all the amino acids found in silk fibroin, the salivary glands may not contain or secrete them. Abderhalden and Dean found the isolation of the silk glands very tedious and so resorted to analyzing whole larvae. In the larvae, as in the silk, glycin and alanin ranked foremost. Glutamic acid was present in the larvae but absent from the silk fibroin. Abderhalden and Weichardt ('09) analyzed the monoamino acid concentration of mature larvae and of adult *Bombyx mori* immediately after emergence. The results (Table 2) are expressed in per cent of the dry weight of fibroin alongside those from larvae about to pupate.

TABLE 2

Analyses of monoamino acids of mature larvae and of adult Bombyx mori

MONOAMINO ACID	LARVA	ADULT
Glycin	10.2	3.5
Alanin	8.7	3.2
Valin	1.7	1.7
Leucin	4.8	8.5
Aspartic acid	1.6	2.7
Glutamic acid	3.5	5.7
Phenylalanin	2.4	2.7
Tyrosin	4.3	1.6
Prolin	1.5	4.0

Leucin, aspartic acid, phenylalanin, and prolin are present in relatively greater amounts in the adult. It is a pity that Abderhalden and Weichardt did not make their analyses on newly formed pupae instead of on newly emerged adults. Their results then would have produced evidence for what they were trying to find, namely, if the monoamino acids of the fibroin are discharged directly as such

by the larvae. Future work along such lines should make analyses of the content of individual monoamino acids of isolated salivary glands removed from larvae shortly before spinning and of the individual monoamino acid content of newly pupated silk worms. The results of Abderhalden and Dean and of Abderhalden and Weichert do show one thing, however, and that is, that it is not *necessary* to assume that any of the monoamino acids present in silk are produced subsequent to the discharge of the silk.

Oku ('29–'35) showed that the various cocoon colors (white, yellow, green, orange, red, etc.) are due to Mendelian races. The yellow color, which is most common, is due to xanthophyll ($C_{40}H_{56}O_2$; melting point = 193°C.). The fading of color is due to the oxidation of xanthophyll to $C_{40}H_{56}O_{15}$ (melting point = 90°C) which, because of its lower melting point and relatively high volatility gives cocoons their well known odor. The xanthophyll from cocoons and mulberry leaves is identical (Oku) and Jucci ('30, '36) and Buonocore and Malucelli ('34, '36) indicated that the yellow pigment of the blood is absorbed by the silk glands and thus gives silk its color. The strong evidence for this was corroborated by Malucelli ('35, '36) who found that the salivary glands are permeable to carotinoids. The red color is also due to a xanthophyll and the green to a pigment termed by Oku bombycin ($C_{20}H_{19}O_7N$). The latter is not present in mulberry leaves but is considered to be derived from the isoquercitrin of these leaves.

F. SILK FROM GLANDS OTHER THAN LABIAL

1. Insects

Embiids have silk glands and silk orifices in their fore legs (Weber, '33). Berlese is cited by Weber as having observed silk glands in the posterior end of the abdomen of coccids.

A most remarkable instance in this respect is that offered by the neuroptera where certain cells of the middle portions of the Malpighian tubes of the larvae are so modified as to be silk-secretory. The first case of this kind was described by Anthony ('02) in the neuropteron, *Sisyra*. The cells which produce the silk (present in the middle portion of the Malpighian tubes) are much larger than ordinary Malpighian tube cells, are vacuolated, and have the branched nuclei characteristic of the silk glands of *Bombyx mori*. These cells degenerate during the pupal period and are not present in the adult. The condition in the neuropteron *Myrmeleon formicarius* (Łoziński 'II, '21) is even more interesting since, at the approach of pupation, the excretory cells themselves, throughout the Malpighian tube, hypertrophy markedly and their nuclei branch out so that the cells eventually appear like regular silk-secreting cells. When spinning is over the nuclei decrease in size by losing particles into the cytoplasm and the cells once more become excretory. The silk passes to the exterior through the hind gut and through a spinneret present at the terminal part of the alimentary canal. Physico-chemical studies have not been made upon this silk.

2. Spiders

(*a*) *Properties*. (i) The "drag-line". We have Benton's ('07) as the sole physical study, from a quantitative standpoint, on the properties of spider silk. He says that his finding of a spider thread of extraordinary thickness (0.01 cm. in diameter; 2.5 meters long) induced him to measure its properties. The thread was composed of several hundred component fibers, each of about 0.005 cm. in diameter,

adhering very loosely. The mean value of the tensile strength was 1.8×10^9 dynes/sq. cm.; this value is close to that of *Bombyx mori* silk fiber (3 to 5×10^9 dynes/sq. cm.). The elongation at rupture was about 20 per cent of the original thread. The elongation produced, however, varied from day to day probably, as suggested by Benton, due to variations in moisture content. The specific gravity was 0.66, or about half that of *Bombyx* silk and two-thirds that of water, which suggests that air is probably entrapped in the silk fibers of spiders. In fact, the low specific gravity of spider silk helps spiders, after the manufacture of a tuft of silk, to soar in the air. The spider thread that has been described above was probably a "drag-line" thread. Comstock ('12), however, stated that drag-line threads, while comparatively large, are usually composed of only two fibrillae and not of several hundred. As its name implies, the "drag-line" thread is that which is secreted by spiders as they move from place to place. They also secrete such a thread when dropping from elevated positions and form the foundations of orb webs with such (Comstock).

TABLE 3

Chemical analyses of spider's silk

Glycin	35.13
d-Alanin	23.4
l-Leucin	1.77
l-Prolin	3.68
d-Glutamic acid	11.7
l-Tyrosin	8.1
Arginin	5.24
NH_3	1.16
Total	90.4

The only chemical analyses of spider's silk are, as far as I know, those of Fischer ('07) on the large spider, *Nephila madagascariensis*. The results are expressed in per cent of dry weight in Table 3. The composition is similar to that of silkworm silk except for the relatively high glutamic acid content (silkworm silk having none) and the greater relative content of prolin. Unlike *Bombyx* silk, there is no serin or phenylalanin.

(ii) The "viscid thread" of Comstock is the spiral line forming most of the orb of orb webs. It is capable of far greater stretching than the 20 per cent value for the drag-line. When touched lightly it adheres. Its considerable elasticity prevents its breaking and makes it a suitable trapping portion of the web.

Figure 2 is a drawing from a photograph taken from Comstock's spider book showing a viscid thread under enlargement. It is composed of an axis thread consisting of two strands and of a series of spherical drops borne upon such. The axis forms the elastic element and the drops form the sticky portion. Comstock noted that the viscid, or sticky, silk is secreted on the axis in a continuous sheath, or cylinder, but breaks into drops immediately the axis is fastened to another radius of the web. The axis is then released from tension with the result that there is a massing of the viscid coat in the formation of drops. Why the viscid layer should mass into several drops and not into a single large drop, why these drops should be regularly spaced cannot be completely explained at present. It will be noted that small and large drops alternate. This may be "explained" by supposing that, as the viscid mass breaks into large drops of equal volume, a little remains over and this little becomes the small drop between the two large drops. Similar surface tension phenomena have been analysed by Guye and Perrot ('03) and Hauser *et al.* ('36).

(iii) "Attachment discs" are composed of silk which attaches the drag-line at intervals to objects (Comstock).

(iv) "Hackled bands" are flat and ribbon-like, and are composed of two parallel threads covered by an amorphous sheath (Comstock).

(*b*) *Production.* "The glutinous fluid of which the thread or web of spiders is drawn", said Lister (1671), "is contained in a pair of undulated receptacles in the abdomen". Comstock ('12) has listed seven known types of silk glands in spiders. Three types of glands have been found in all species and a fourth is wanting in only two families.

Many spiders have the capacity of "shooting" out the discharged silk. When such a mass of silk is attached to the spider it may be carried aloft by wind (Ray, 1670; Lister, 1671; Bon, 1721; Comstock, '12). The wind may fasten the threads to some nearby object or, in the other extreme, carry the spiders hundreds of miles from land where they may be met by ships (Comstock).

How certain spiders manage to form orb webs of exquisite symmetry as yet requires thorough analysis. A step in this direction has been made by Peters ('36–7).

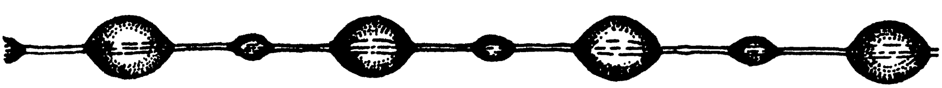

FIG. 2. SPIRAL THREAD FROM THE ORB-WEB OF ARANEA

[From a photomicrograph by John Henry Comstock in *The Spider Book* (copyright 1912), through the courtesy of Doubleday, Doran and Company, Inc.]

G. NAUPLIUS EYE SECRETION

Kollmann ('24) found that the single median eye of the nauplius stage persists in the adults of phyllopod Crustacea in a rudimentary state. The retina of this eye was interpreted as being, in the adult, glandular in appearance even though still connected with the supraesophageal ganglion. Kollmann did not determine whether this eye has the photoreceptive functions, which it possesses in the nauplius larval stage, but there were no rhabdomes.

H. HORMONES

(*a*) *From the eyestalks of Crustacea.* It is probable that the cells secreting hormones in the eyestalks of Crustacea, which cause pigment contraction, are modified hypodermal cells (Koller, '27, '29).

(*b*) *From the oenocytes.* These are relatively large cells of ectodermal origin found in insects and which may be confined solely to the hypodermis or, also, be present in the haemocoele, depending upon the species. In the bug, *Rhodnius* (Wigglesworth, '33), they are newly formed at each molt, except the last, from undifferentiated cells lodged in the hypodermis. In this bug they reach their maximum size just before the new cuticle is laid down after each molt. They, unlike the exuvial glands, persist in the adult stage. After the feeding of the adults they become active in the females but very little so in the males. Judging from their behavior under these particular conditions, Wigglesworth suggested that the oenocytes may be involved in the synthesis of some of the non-chitinous constituents of the cuticle and in the synthesis of the egg shells.

(*c*) *From the corpora allata.* The corpora allata of insects are usually two small whitish bodies on either side of the stomodeum behind the supraesophageal ganglion and are innervated by the stomodeal nervous system. In some cases they are united into a single body on the dorsal

surface of the stomodeum directly behind the supraesophageal ganglion (Maloeuf, '33). According to the Snodgrass ('35), so far as known, they occur in all insects and are included in this section because they are of ectodermal origin. Wigglesworth ('34a, '34b, '35) has indicated, in *Rhodinus*, that they are ductless glands which secrete at least two hormones which initiate molting, inhibit the onset of metamorphosis, and induce egg formation. By noting cyclical changes, Burtt ('37) indicated and, by transplantations, Hadorn ('37 a, b) demonstrated that the "ring gland" (corpus allatum) of flies is the normal agent which induces the metamorphosis of these creatures.

On the other hand, Caspari and Plagge ('35), Bounhiol ('36 a, b), and Plagge ('38) have, as a result of the use of transplantation and without overlooking the existence of the known "critical" period, came to the conclusion that the brain itself and not the corpora allata induce the metamorphosis of moth larvae (*Sphinx ligustri*, *Deilephila euphorbiae*, *Bombyx mori*). Weed ('36 a, b; '37) found that complete elimination of the corpora allata from grasshoppers at any time during the last larval stage does not prevent the final molt into the adult. This last evidence, however, does not speak either way since the critical period is not known and since there may have been, remaining from the penultimate molt, an adequate amount of molting hormone in the blood. Weed has nevertheless demonstrated that the corpora allata of the grasshopper are essential for complete growth of the oocytes and for the occurrence of the oviducal secretion which mutually attaches the eggs into a "pod." It is noteworthy that Schrader ('38) could find no evidence of a secretory function in the cells of the brain of the moth, *Ephestia kühniella*, at any time but, rather, observed that the corpora allata of this insect display secretory activity during metamorphosis.

1. LIGHT PRODUCTION

Under this topic only light-producing cells of ectodermal derivation will be described. Koch ('27) dug out of the soil a centipede, *Scolioplanes crassipes*, and found that it emitted an intense green light from its sterna (Fig. 3). When it moved it left behind it fluid droplets (Fig. 4A), exactly corresponding to the "pores" of its sterna, which emitted green light. This shows that both the luciferin and

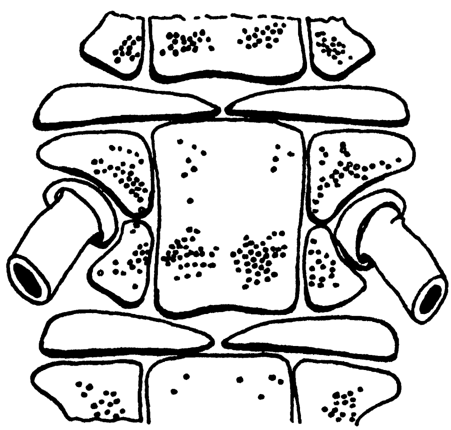

FIG. 3. STERNA OF SCOLIOPLANES CRASSIPES
The pores of the unicellular light organs are on the sterna of the trunk segments. 41.3×. After Koch.

the luciferase is secreted to the exterior. This would require a perforation at some point in the membrane of each secreting cell, since luciferase is a complex protein. Each light organ of this animal is a large hypodermal cell 140μ long, 45–70μ in diameter, and with a relatively small nucleus 5μ in diameter (Fig. 4B). The so-called "pores" are the external appearance of a thin invagination of the sternal cuticle into each light-producing cell. Similar conditions are present in *Geophilus linearis* and other members of the group Geophilidae of the Chilopoda.

Sudden submersion in water, a weak in-

duction current, chloroform, or a rise in temperature of 10°C. stimulated the production of light in *Scolioplanes*.

The organs are equally developed in both sexes and luminesce all the year round and not only, as some have believed, during the breeding season. The fact that all the Geophiloidea are eyeless also indicates that luminescence in these forms does not serve for sex attraction (Cook). The significance of the light producing cells in the economy of their possessors is indicated in the following topic.

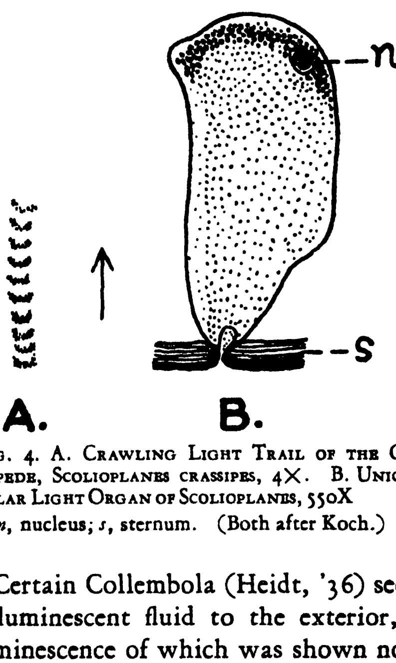

Fig. 4. A. Crawling Light Trail of the Centipede, Scolioplanes crassipes, 4X. B. Unicellular Light Organ of Scolioplanes, 550X *n*, nucleus; *s*, sternum. (Both after Koch.)

Certain Collembola (Heidt, '36) secrete a luminescent fluid to the exterior, the luminescence of which was shown not to be due to bacteria. Phylogenetically, it is of interest to note that the luminosity of certain terrestrial oligochaetes (Walter '09) is produced by the secretion of hypodermal glands.

J. HYDROCYANIC ACID

Guldensteeden-Egeling (1882) showed that the diplopod *Paradesmus gracilio* secretes benzaldehyde-free hydrocyanic acid. The repugnatorial glands which produce the secretion "open," according to Weber (1882), near the median dorsal line of certain segments and it is only from these segments that the characteristic odor of HCN is diffused. Wheeler (1890) noted that *P. virginiensis* acts in the same manner when roughly handled since the Prussian blue test for the —CN radical was positive.

It has been mentioned above that the groups Geophiloidea of the Chilopoda have ventral unicellular glands the secretion of which is luminous and leaves a trail. Since all Geophilidae are eyeless this luminosity cannot be supposed to serve for sex attraction. The luminous secretion has the odor of HCN (Cook) so that the significance of the light-producing cells of these animals appears to be repugnatorial. Luminosity, therefore, apparently has no adaptive significance in these cases and is merely an inevitable phenomenon arising from established physiological processes.

K. CAMPHOR

This substance is a benzol derivative with a ketone group and its detection among the synthetic secretory products of animals would be an extreme exception.

In 1900, Cook detected the odor of camphor from secretions of hypodermal glands of the terga of the trunk segments posterior to the first four trunk segments of the millipede, *Polyzonium rosalbum*. The camphor is discharged upon mechanical stimulation and may be exuded from the pores in thin milky threads which harden on exposure to air. No actual analyses, other than by smell, were made of this substance, but the statement of Oscar Loew was obtained that "there is no substance recorded with an odor that is likely to be mistaken for that of camphor." Cook has noted that another order of diplopods, the Merochaeta, secretes hydrocyanic acid from glands which are apparently homologous with the

camphor glands in other forms. These glands are apparently protective, for, in general, diplopods are comparatively immune from mites and other parasites which commonly attack animals of similar habitats.

Cook has shown that these animals will soon die (presumably of their own camphor fumes) when sealed in a small bottle and therefore are not immunized against their own toxins. Since camphor is not a protein and since an antitoxin can be produced by the body only against a substance which possesses a protein grouping, it is clear why the animals possess no antitoxin for the poison they secrete. In this respect they differ from scorpions, spiders, and centipedes, the blood of which apparently contains the respective antitoxins for the toxins secreted.

L. FORMIC ACID

1. Lepidoptera

The cocoons of many Lepidoptera are hard and horny in texture. In 1897, Latter found that larvae of *Dicranura vinula* have thoracic glands which open, by an orifice, at the prothoracic sternum. These glands were noted to secrete formic acid and thus give the silk of the cocoon its characteristic horny texture. The freshly made cocoon is consequently strongly acid when still moist. The silk threads in contact with the walls of a glass container on which sodium carbonate had been painted remained loose and soft while the threads in the interior of the cocoon became horny as usual. Adding formic acid to the silk glands caused the whole mass to swell and set in a firm and nearly transparent jelly. When this jelly was exposed to the air it shrank and yielded a hard horny mass very closely resembling a portion of a cocoon in texture and appearance. The cocoon of *Dicranura* is, in this way, made very tenacious and waterproof. Presumably, the formic acid is normally applied to the silk fibers as the the latter are secreted and these harden on contact with the air. The silk glands of *Lymantria cossus*, on the other hand, remained flaccid when treated with formic acid thus showing that the occurrence is not general among cocoon-spinning Lepidoptera. The fluid of *D. vinula* and *D. furcula* is 40 per cent formic acid (Poulton, 1887) and is discharged by the larvae when handled. For a description of this subject from a naturalists viewpoint, see Shelford ('03) and his quoted predecessors and the review by von Fürth ('03).

2. Hymenoptera

Stumper ('22 a and b) made analyses of the formic acid concentration of the poisonous secretion of the females (workers and queens) since the males do not have stings. All eleven species of the family Camponotinae contained formic acid. *Formica rufa* was foremost with the extraordinary value of 18 per cent formic acid. *Lasius fuliginosus* was lowest with a value of 2.3 per cent. The formic acid content of the venom secretion of *F. rufa* varied from 21.35 to 72.80 per cent. Probably atmospheric moisture influences the amount of water in the secretion. In fact, Melander and Brues ('06) had found that ants of a given species kept in very dry weather have a higher concentration of formic acid in their secretion than those under usual conditions.

A temperature relationship of the amount of formic acid secreted was noted by Stumper who obtained a Q_{10} value of 2.16 within ordinary temperature range. This implies that for every rise of 10°C. (within normal limits) the amount of formic acid present in the venom secretion

is more than doubled. This closely corresponds with van't Hoff's value of ca.2 for the speed of chemical reactions.

In no case did Stumper find formic acid in the bodies of worker ants of the families, Myrmicinae and Dolichoderinae. The harmful effects, produced on man, of the venom of the latter was, therefore, ascribed to some other unknown substance. All the free acid in the liquid of the poison glands (situated in the posterior end of the body) of queen and worker *Formica rufa* and *Cataglyphis bicolor*, with the exception of possible negligible amounts, is formic acid (Stumper, '22c).

The poison reservoir of bees and wasps contains a liquid that is acid to litmus (Heselhaus, '22). The honeybee poison is not due to the formic acid constituent since when such is removed the poisonous effect is not reduced (Langer, 1896, 1899).

M. OTHER "TOXINS"

In several of the works to follow no attempts were made to localize the anatomical source and chemical nature of a toxin and their inclusion in this review is only a matter of course. The terms "toxin" and "venom" *are capable of wide application since most foreign proteins are harmful, in a greater or lesser degree, and many rupture the red blood corpuscles of vertebrates.* A rigorous study of the chemical nature of the poisons *secreted* by invertebrates awaits the future. For a review of the natural history of this subject see von Fürth's ('03) book. The treatises on venoms by Calmette ('08) and Phisalix ('22) have a great deal that is interesting and yet much which should be subjected to test by modern methods.

1. Bees and wasps

Phisalix (1897 a, b, and c) injected a glycerin extract of whole wasps (*Vespa crabro*) into the thigh of a guinea pig. The result was lowering of the temperature of the guinea pig through 4°C. for about 36 hours and accompanied by local swelling. When injected in suitable amounts the liquid from the poison vesicles of wasps was found to exert an antagonistic effect on viper venom (species of viper not stated). Death of the guinea pig under such conditions was either prevented or retarded. The "toxin" of this wasp is not destroyed by heating at 120°C. for 20 minutes. After such treatment it will still immunize versus viper venom. It is soluble in ethyl alcohol and in chloroform. Tests for alkaloids in such solutions are negative. The alcoholic precipitate of the fluid in the poison reservoir of the wasp produces no symptoms and has no immunizing power. The "toxin" is, therefore, not an alkaloid nor a protein. Phisalix ('05) injected 1 cc. of a distilled water extract of bees' eggs (concentration and species not stated) into the thigh of a sparrow. The leg became paralyzed and death resulted in three days. Phisalix concluded that in bees, as in vipers, venom accumulates in the ovules. There is, however, among spiders, no relationship between the "venom" in the eggs and that of the poison glands. This may also be true for bees.

2. Aphids (Pelargonium sp.)

Dewitz ('15) found that 1 gm. (wet weight) of this plant louse will rupture the red blood corpuscles of 25 cc. of defibrinated and undiluted ox blood.

3. Aquatic beetles

Portier ('09) concluded that *Dytiscus* larvae must secrete a poison which is rapidly fatal to larger prey than themselves e.g. small fish and salamanders.

4. Centipedes and millipedes

Lithobius forficatus is apparently immune to the "poison" in its mandibular glands

because of the presence of an antitoxin in the blood. Levy ('27) seems to have demonstrated the presence of such an antitoxin by injecting blood of the centipede into crayfish which had been bitten by it. It was similarly demonstrated that the blood of *Lithobius* is antitoxic to the toxin of a centipede belonging to another genus. In 1913 Corson-White reviewed the investigations demonstrating that poisonous reptiles are immune to their own poison and more or less so to the poison of other species and that non-poisonous reptiles are relatively resistant to the poison of poisonous reptiles. The fact, however, that she could not demonstrate the presence of an antibody in the blood of the Gila monster, *Heloderma suspectum*, makes it necessary to treat Levy's work with caution until confirmed.

The poison of *Lithobius forficatus* (Karlinski, 1883) contains formic acid or an aldehyde. The toxic action of the millipede *Iulus terrestris* (Béhal and Phisalix, '00) poison is akin to that of quinone. Liebermann's quinone test was positive but there was not sufficient material to isolate and analyze the quinone. When *I. terrestris* (Phisalix, '00) is held it rolls up and discharges a yellow penetrating fluid with a piquant odor through the orifices of ventral hypodermal cells.

Testing the effects on himself and on rats, Baerg ('24, '34) showed that *Lithobius mordax*, *Theatops spinicaudus*, *Scolopendra heros*, and *S. polymorpha* (centipedes) are harmless. In fact, Cornwall believes that the "venom" of a centipede serves principally as a digestive fluid (Pierce, '21).

5. Spiders

The poison glands are situated in the anterior part of the body and open near the tip of the claw of the chelicera of the corresponding side. The glands lead to a poison reservoir which is surrounded by muscle fibers. The poison of most spiders is effective only in causing the paralysis of captured insects and small arthropods.

In 1693, the Italian physician, Sanguinetti (Kobert, '01), allowed himself to be bitten on the arm by tarantulas in the presence of a witness. The results were no greater than a mosquito or ant bite. In some cases secondary infection set in. In fact, the large tarantula, *Trochosa singoriensis*, would not bite even when induced to do so. Concerning the notorious tarantula bite, a physician, Cirillo, told the Royal Society of London, in 1770, that ". . . neither men nor animals (meaning large mammals), after the bite, have had any other complaint, but a very trifling inflammation of the part, like those produced by the bite of a scorpion, which go off by themselves without any danger at all." The bite of the tarantula has been supposed to be cured by music. "Every year," Cirillo went on, "this surprising disorder loses ground, and doubtless in a very little while it entirely loses credit."

In 1882, Blackwell allowed himself to be bitten by seven different species of spiders (apparently all natives of Great Britain). The bite produced no effects on himself nor on spiders of the same or other species, beyond those similarly caused by a pin. Bites had destructive effects on insects but not with the virulence or rapidity that had been supposed. Concerning the notorious black widow spiders (*Latrodectus mactans* mature females) an early missionary, Sahagun, wrote: "There are some spiders in this country, they are black and have a reddish tail. The stings cause great fatigue for three or four days, although they do not kill with their sting" (Curran, '37). "I have endeavored," wrote Comstock ('12), "to trace to their source some of the newspaper stories of terrible results following the bite of a spider; but have not found a bit of evidence that would connect a spider with the injury in any one of the cases investigated." He assures us that there is no spider in the northern part of the United States which is capable of producing injury to a human being by a bite. W. Riley and Johannsen ('15) adopt the same attitude. C. Riley and Howard (1889) had recognized an exception and considered that certain fairly small spiders of the genus *Latrodectus* (black widow) may "exceptionally and depending upon exceptional conditions, bring about the death of a human being" —a thoroughly non-committal statement based on rather meager facts (Kobert's, '01 experiments on the effects of extracts of the whole cephalothoraces of the spiders on cats). The statements of Kobert ('01)

and D'Amour *et al.* ('36) that the poisonous principle in the secretion of *Latrodectus* is a protein are not conclusive since they used extracts of the whole gland. In fact, Kobert thus found the "venom" not only in the poison glands but also in other tissues and even in the eggs. D'Amour *et al.*, however, state that the venom of the eggs is different.

In certain contrast to the above direct quotations Bogen ('32) and D'Amour *et al.* ('36) have pointed out that the bite of mature females of *Latrodectus mactans* (the immature females and males are relatively or quite harmless to rats even though they possess poison glands) often produces excruciating pain and muscular spasms throughout the trunk which last for about three days (see also Baerg, '22 and Blair, '34) and that the bite has, in some cases, proved fatal to human beings. The extent of the effect, of course, varies with the amount of poison injected and with its concentration. When given as food, however, the poison glands are quite harmless even to small animals such as birds and rats (D'Amour *et al.*). *L. mactans* occurs throughout the United States and it has been observed in many parts of Canada. It is quite common in the southwestern states where it often proves a pest.

The experiments of Blackwall (*loc. cit.*) and of Levy ('27) indicate that spiders have an antitoxin in their blood which is also effective against spiders of another species. Sachs' ('02) "antitoxin" versus spider venom is of no importance because whole-spider extract was used.

Erythrocytes of the rat and rabbit are very sensitive to extracts of the house spider (*Araneus diadematus*), undergoing complete rupture; those of the mouse, goose, and man are less sensitive; and those of the guinea pig, horse, sheep, and dog are apparently insensitive (Dewitz, 15).

Egg extracts of *Araneus erythromela*, of *A. amaurophyla*, and of *Latrodectus poliostoma* (Houssay, '16) have powerful haemolytic action. The haemolytic power of the newly hatched animals was a little less than that of the eggs. Extracts of the female spiders containing eggs had haemolytic action but those of adult male spiders produced no haemolysis. Houssay concluded that haemolysis can have nothing to do with the poison secreted by the poison glands since males and females with or without eggs, paralyze captured flies with the same speed. The haemolysins are apparently proteins since they are non-dialyzable; are absorbed on animal charcoal; and are insoluble in methyl, ethyl, or amylic alcohol, benzine, chloroform, and acetone. The serum of a rabbit immunized against *L. mactans* will only neutralize the haemolysin of *L. mactans*. The serum of rabbits immunized versus *A. erythromela* or *A. amaurophyla* will neutralize the haemolysin of either. The intrageneric specificity displayed by the anti-haemolysin is further evidence for considering the haemolysins as proteins. Houssay found no haemolytic action in the eggs, young, or adults of *Lycosa poliostoma*, *Polybetes pythagorica*, *Theridion calcinatum*, and *T. uber*.

Levy ('16) corroborated one of Houssay's conclusions by showing that the "venom" of the chelicerae of spiders has no haemolytic action on rabbit, mouse, and frog erythrocytes and that it, furthermore, has no toxic action on these vertebrates. The "venom" was, however, very toxic to crayfish, producing paralysis followed by death. The toxic action of the secretion was destroyed by heat at about the same temperature as that for protein coagulation. This contrasts with the toxin of wasp venom (see above), which is not a protein. Levy showed that the toxic or haemolytic action of the eggs is not correlated with the degree of toxicity of the venom produced by the

poison glands in the chelicerae. Thus, the venom of the poison glands of *Tegenaria parietina*, a species containing no toxic substance in its eggs, is just as toxic as other species. The blood of tenerid spiders has antitoxic properties against the spider venom and this antitoxicity was, in fact, stated to be elevated by heating the blood to 72°C. We do not know how this antitoxicity is exerted. Antitoxicity could not be shown in the blood of the Epeiridae.

Levy came to the rather unwarranted conclusion that, because the toxin secreted by the poison glands of the spiders was deadly to the crayfish but harmless to the rabbit, mouse, and frog, it is specifically adapted to the destruction of arthropods—their "natural enemies"— and that the antitoxic properties of the blood of these spiders towards their own poison is because they feed almost exclusively on arthropod tissues. He, however, apparently tested the effects of spider venom on only one arthropod.

6. Scorpions

These include the most poisonous arthropods. Their poison glands are located at the posterior extremity of the abdomen where the secretion is discharged by way of the sting.

During the dry season, according to Barrett ('01), the sting of a given species is much more toxic than during the rainy season. Judging from observations on the effects of dryness and humidity on the formic acid concentration of the toxic fluid secreted by ants, it is very probable that this may be due to a greater concentration of the toxin during the dry season. Bites by the Egyptian species, *Buthus quinquestriatus* (Wilson, '04), are fatal in about 60 per cent of the cases under five years of age. For the most venomous Mexican species (*Centruroides suffusus*, *C. limpidus*, and *C. noxious*) see Cavarox (1865) and Baerg ('24, '34). By tests on himself and on rats Baerg (*loc. cit.*) was able to show that *Centruroides vittalus*, *C. thorelli*, *Centrurus exilicauda*, *Vaejovis variegatus*, *V. subcristatus* are almost harmless.

The large brownish-yellow genus, *Centrurus* sp., (or "alacran huero") of Mexico is perhaps the most poisonous arthropod (Barrett). Deaths caused by scorpion stings among children under six years of age were common in the Mexican states of Durango and Guerrero when Barrett wrote. Launoy ('01) allowed rats, mice, birds, frogs, and sparrows to be stung. On the whole the frogs were least irresistant but were nevertheless overwhelmed. Launoy made histological studies of the victims which died within ten minutes. In all cases the lesions were quite similar. There was extensive destruction of the glomeruli of the kidneys, vacuolization of the cytoplasm of the cells of the convoluted tubes of the kidney along with chromatolysis and karyolysis.

Levy ('27) quotes Metschnikoff as showing that the blood of a scorpion has antitoxic properties against the venom secreted.

7. Whip "scorpions" and Solpugida

The "poisonous" effects of *Mastigoproctus giganteus* (or "vinaigrillo") of Mexico has been described by Barrett ('01) in the following incidents:

> "At Cuernavaca I was told of a field labourer who was found dead, but *sitting bolt upright*, so great had been the nervous shock and muscular cramping from a 'vinaigrillo' sting." Comstock (1897), however, had previously written: "Although it has been stated often that their bites are poisonous, we can find no direct evidence that it is so. They destroy their prey by crushing it with their palpi." The incident related to Barrett may, therefore, be the outcome of the awed imagination of uneducated peasantry. In fact, Petrunkevitch (*In Litt.*, '37) has informed me that whip "scorpions" have no poison

glands. With reference to the whip-scorpions, "Dr. Marx states that there is neither a poison gland nor a pore in the claw of the chelicera" and Dr. A. Walter stated that the Solpugida are not poisonous and have no poison glands or pores in the fangs.

N. MATING GLANDS OF FEMALE MOTHS

Male moths are often attracted to females of their species, or to extracts of the females, from considerable distances. The female apparently secretes the volatile gas-producing liquid from certain unicellular hypodermal glands in the posterior region of the abdomen. Her abdomen is alternatively protruded and retracted when the males are being attracted (Eltringham, '33). Olfactoreption is the chief means of communication among social insects (cf. McIndoo, '28). They so distinguish between individual, sex, hive or colony, and trail odors. Scent glands are located on various parts of the body.

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STUDIES ON THE EVOLUTION OF SOME DISEASE-PRODUCING ORGANISMS

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INTRODUCTION

MAN has been able to unravel the evolutionary history of many of the plants and animals which have left fossil records in spite of the tremendous difficulties of the task. He has not met the same degree of success in tracing the origins and evolutionary histories of the disease-producing organisms. Not only did most of these organisms fail to leave any fossil records, but even if they had left good fossil specimens the latter would probably serve us only in a very limited way in making the type of comparison in structure which has been possible in the more complicated animals and plants. In size and external appearance many of the disease-producing organisms are remarkably similar today, even though we can subject them to staining and careful microscopic study. It, therefore, seems very unlikely that the study of fossils will ever have more than a very limited value in uncovering to us knowledge of the early history of these forms. In fact, we have to face the possibility that this early history will forever be obscure to us. The records of the fossils have been and will continue to be helpful in connecting our present knowledge of the organisms, or more particularly of the effects produced by them, with the comparatively recent progenitors of these organisms. It would seem, however, that we shall have to turn to other sources for our ideas about the more remote stories of their past. We have but one choice in this matter. We must seek for this knowledge in the study of living forms. The study of parallel evolution of hosts and parasites has already been an effective tool in clarifying the evolution of both parasite and host when used by such investigators as Kellogg (1913), Metcalf (1923) and Darling (1920). I propose here to use this method to the limited extent possible with our present knowledge in attempting to reveal some of the evolutionary histories of certain disease-producing organisms which are transmitted by arthropods. I am particularly interested in attempting to apply the method to forms having obligate heteroxenous life-cycles. The origin of parasitism in these forms which no longer have any free-living stages presents a particularly fascinating problem. The purpose of this discussion is not so much the presentation of convincing proof of the evolution of these parasitic forms as it is to direct attention to the possibilities of this method in developing this field. The few facts which we now have which may be used in the development of any theory of evolution have resulted from investigations motivated by other desires. It seems only reasonable to suppose that when the desire for adding to our stock of knowledge on the evolution of these forms becomes strong enough, the facts bearing upon this field will be sys-

tematically sought instead of being picked up by the wayside as at present.

In approaching the evolution of the organisms concerned in the production of disease in man and the higher animals, we first face the problem of examining the history of these organisms immediately before becoming human parasites. This question is easily answered in the case of many diseases. We know, for example, that bubonic plague is a disease of rodents which is only incidentally transmitted to man. Undulant fever is a human disease because of the habit of drinking cow's milk. Trichinosis becomes a human parasite whenever man ingests insufficiently cooked pork which is infected with *trichina*. Balantidiosis becomes a serious human disease when the parasite, which ordinarily lives in pigs and apparently produces no ill effects upon the pigs, gains entrance by contamination to the human alimentary tract. The list of such diseases could be greatly extended. They are listed here only as examples of organisms ordinarily affecting other animals which produce disease under special circumstances when introduced into the human body. Such examples serve, however, only to push the problem back one more step. We immediately then face the similar problem of determining where these organisms were before they were parasites of the animal mentioned.

Undoubtedly the disease-producing organisms had very diverse origins. Some could conceivably have been parasitic from the beginning of their existence since certain cells of an animal might have broken loose and assumed an independent existence. They would then have evolved along with their hosts, either changing hosts several times during their evolution or becoming so well adapted to transmission to the progeny of the host that they remained restricted to a given species, or to the species evolving from it. Others were free-living at first and then became adapted to life in one or more types of host. Such parasites as the hookworms show in their life-cycles now the evidence of such a transition, for during the early part of the life-cycle the hookworms are similar in habit to free-living nematodes. In many cases there is insufficient evidence at present to decide what was the probable course of evolution. For example, I should be reluctant at the present time to venture a guess as to whether *Pasteurella pestis* developed first as a parasite of fleas and then of rodents or whether the reverse happened. I intend to discuss only such groups of parasites as seem to me to show good evidence of their origins. My chief interest lies in the group of diseases which are borne by arthropods. Consequently I shall discuss only some of the arthropod-borne infectious agents. I do not wish to be misunderstood as hypothecating that all parasites of man and higher animals had arthropod origins.

ORIGIN OF RICKETTSIAE

The rickettsiae cause diseases of man, such as typhus fever, trench fever, Tsutsugamushi disease, Rocky Mountain spotted fever and the closely related diseases such as *Fièvre Boutonneuse*, São Paulo fever, Eastern spotted fever, and diseases of animals such as heartwater of sheep. The causative organisms of diseases of this kind are all known to be transmitted by arthropods, and the evidence in relation to their evolution is such as to seem incontrovertibly in favor of the belief that they are primarily parasites of arthropods and secondarily of vertebrates. Not only are all of the rickettsiae of vertebrates known to parasitize arthropods also, but a great many similar forms occur in other arthropods and are apparently not parasitic in higher animals. (See Cowdry, 1923,

and Hertig and Wolbach, 1924.) Furthermore, most of them are so well adapted to their arthropod hosts that they no longer produce disease in the arthropod. This close adaptation is also shown in their ability to pass from the infected female arthropod into the egg and thus to the next generation of arthropods. *Rickettsia prowazeki*, which produces typhus fever in man, is exceptional in that it does bring about destruction of the tissues of the louse by which it is transmitted, and also by the fact that it does not pass into the egg of the louse and hence is not hereditarily (congenitally) transmitted to the next generation of lice. Both of these exceptions point toward the relatively recent parasitism of lice by this organism. In fact, there is much reason for believing that in common with other rickettsiae the causative organisms of typhus are of acarinid origin and that they have been lately and poorly adapted to parasitism of body lice of man. Our knowledge of the murine or endemic type of this disease helps to complete the story of its probable origin. This is a rodent disease transmitted by means of rat fleas and rat mites from rat to rat and occasionally from rat to man. Dove and Shelmire (1931 and 1932) demonstrated that this rickettsia can pass through the egg of the rat mite, *Liponyssus bacoti*, and infect the next generation of mites. The most logical hypothesis concerning the evolution of these rickettsiae would seem to me to be as follows. The rickettsiae were associated with mites for long periods of geological time; perhaps even back to a time before the mites had taken up the parasitic habit, and during this time they and the mites became so well adjusted to each other that hereditary transmission was established and the rickettsiae ceased to produce disease in the mites. When the mites became parasitic upon rodents the rickettsiae then occasionally parasitized the rodents and at first probably caused serious disease. There may have been a long period then during which the rickettsiae and rodents became better adapted to each other, but during this time the method of hereditary transmission was retained in the mite and still persists today. After this typhus-like disease was established in rats, the rickettsiae were then taken up by other blood-sucking parasites of the rats and, at least in the case of rat fleas, became well enough adjusted to their new hosts to be transmitted by them (Dyer, Cedar, Rumreich and Badger, 1931). The association with rat fleas has not been long enough, however, to permit the development of the hereditary transmission of the rickettsiae through the eggs of the fleas.

Louse-borne typhus of man most probably originated from the murine type. The latter was occasionally transmitted to man—as it still is today—by rat mites and rat fleas. The adaptation to *Pediculus humanus*, the human body louse, then began and has progressed to the point that typhus can now be carried on in epidemic form by the lice. The rickettsiae are still pathogenic for the louse and have not yet established the hereditary type of transmission. The high pathogenicity for man also indicates that the disease has only recently become a human disease. The term, recently, however, must be interpreted in a geological, relative sense. Zinsser (1935) has clearly shown that the disease is probably very ancient in man in a historical sense.

Trench fever, which is also caused by a rickettsia, may have had an evolutionary history similar to that of typhus, but with a longer association with man as well as with human lice. The lower degree of pathogenicity for each of these hosts would favor this hypothesis. On

the other hand, the rickettsiae which produce trench fever may have been parasites of lice originally and hence not closely related to the rickettsiae of typhus and the typhus-like diseases. Tsutsugamushi disease which would seem to be distinct in many characteristics from both typhus and spotted fever is transmitted by *Trombicula akamushi* which is also an acarinid. If we include trench fever in the typhus group on the first hypothesis or exclude it on the second hypothesis, we find that all of the typhus-like diseases of man and also heartwater of sheep are associated with Acarina and most probably were parasites of ticks or mites originally.

ORIGIN OF CERTAIN SPIROCHAETES

Certain spirochaetes which produce disease in man and animals seem also to have originated from ticks, and a study of their evolution offers an interesting parallel to that of the rickettsiae above. The forms to be considered here are the blood spirochaetes belonging to the relapsing fever group, and the avian spirochaetes. Relapsing fever exists in man in two forms, endemic and epidemic, the former being transmitted by ticks of the genus, *Ornithodorus*, and the latter by human body lice. The endemic type is primarily a rodent disease which is transmitted by *Ornithodorus* from rat to rat and occasionally from rat to man. To strengthen the parallel with endemic and epidemic typhus, the spirochaetes are hereditarily transmitted from the infected tick through the eggs to the following generation of ticks, and this transmission can continue for at least three generations of ticks and perhaps indefinitely, without the need for receiving spirochaetes from another rat. (Möllers, 1907.) A similar hereditary transmission does not occur in the louse. Furthermore, the method of transmission by the louse is an inefficient one. Infected lice are incapable of transmitting the disease to man either by their bites or through contamination by their feces. The spirochaetes live in the coelomic fluid and are transferred to man only when the louse is crushed on his skin. This is obviously an inefficient means of transmission and, together with the other evidence, practically proves that the relationship between the spirochaete and louse is more recent than that between spirochaete and tick. The analogy with the rickettsiae is further strengthened by the fact that similar organisms produce disease in fowl and that these spirochaetes are associated with ticks. The ticks belong to the genus *Argas*, which is closely related to *Ornithodorus*. The spirochaetes of fowls have even been experimentally transmitted by *Ornithodorus*. Hereditary transmission of the spirochaetes also occurs in the female *Argas*.

It would seem very likely then that these spirochaetes were parasites of ticks of the family Argasidae before the latter began to parasitize higher animals. Nicolle and Anderson (1927), however, conceive of relapsing fever spirochaetes as being parasites originally of small mammals, then of *Ornithodorus*, of man, and finally of lice. That the louse strains and tick strains of spirochaetes are not so vastly different today is shown by the experiments of Nicolle and Anderson (1926) in which the tick strain was transmitted to monkeys by lice. A word of caution is needed against the possible attempt to assign to all spirochaetes an argasid origin. Other groups of spirochaetes have probably followed diverse courses of evolution.

ORIGIN OF PIROPLASMAS

The blood parasites belonging to the sub-order Piroplasmidea are protozoa

which parasitize the erythrocytes of mammals (during part of their life-cycle, at least) but which do not produce the malarial pigment, haemozoin. Some examples are *Babesia bigemina*, which causes Texas cattle fever, *Babesia motasi*, *B. ovis* and *B. sergenti* of sheep, *B. caballi*, and *B. equi* of equines, *B. canis*, *B. gibsoni* and *B. rossi* of dogs, and *Theileria parva*, which causes East Coast fever in African cattle. A large number of other species occur in these animals as well as other less well known forms in bats, mice, rats, hedgehogs, moles, monkeys, antelopes and goats. The piroplasmas are of interest to us here because they represent a high degree of parasitism in both their vertebrate and invertebrate hosts. In all cases where the transmission is known, Ixodid ticks are involved. And as shown by Smith and Kilborne (1893) for Texas cattle fever in the tick, *Boophilus annulatus*, some of these forms are normally hereditarily transferred to the progeny of the infected female. In view of the wide diversity of vertebrate hosts infected with piroplasmas, and of the fact that Ixodid ticks seem to serve as intermediate hosts for all of them, it would seem highly probable that they were originally parasites of ticks. Christophers (1934) has pointed out that the ungulates and carnivora, which are the animals chiefly parasitized by piroplasmas, have tended to roam the plains and lands remote from water. In these habitats there would be greater chance of their coming in contact with ticks.

We do not yet know enough about the transmission and life-cycles of the parasites of the genera, *Anaplasma*, *Toxoplasma*, *Grahamella*, and *Bartonella*, to be able to do more than guess at their origins.

ORIGIN OF THE HAEMOFLAGELLATES

The parasites referred to as "haemoflagellates" belong to the Family Trypanosomidae. Besides containing the well-known forms producing trypanosomiasis and leishmaniasis in man and animals, this family includes a large number of forms in invertebrates, and one genus (*Phytomonas*) which lives in the latex of certain plants. The belief that most, if not all, of these parasites were originally parasites of invertebrates and particularly of insects is fairly general now among protozoologists. The case has been very clearly presented by Adler (1933).

The close similarity of the haemoflagellates of vertebrates with those of invertebrates is the chief reason for the belief that they had an association with invertebrates first. Among the insects there is a very large number of species of the flagellates living as parasites of the intestinal tract and being transmitted from one individual insect to another by fecal contamination. Many of these are indistinguishable morphologically from the cultural forms of trypanosomes or leishmanias which produce disease in man and animals. The flagellates which develop in *Phlebotomus* flies and certain other insects when these are fed upon patients having Kala Azar are identical in appearance with the members of the genus *Leptomonas* found naturally in a large variety of insects and not known to involve any vertebrate in their life cycles. In addition to this evidence, a fairly large number of the species parasitic in vertebrates are known to be transmitted from vertebrate to vertebrate by insect vectors. For example, African sleeping sickness of man and nagana of animals are transmitted by blood-sucking flies of the genus *Glossina* (tsetse flies); oriental sore is transmitted by *Phlebotomus*, Chagas' disease by reduviid bugs, and the non-pathogenic *Trypanosoma lewisi* of rats by rat-fleas. It is very easy to see how these insects may have transferred their flagellates to the vertebrates which they

attacked, and then to have continued to act as vectors of the parasites or to have dropped out of the life-cycle completely. It is believed that the latter may have happened in the case of *Trypanosoma equiperdum*, which causes a venereal disease of equines. This is the only *Trypanosoma* of vertebrates known to be unassociated with an insect. In this case the parasite became adapted to transmission by contact during coitus and when this adaptation was perfected the need for the original host—perhaps a biting fly—no longer existed.

It might, perhaps, be argued that the diversity of invertebrate hosts for the haemoflagellates of vertebrates is against the belief in the invertebrate origin of these protozoa. The known vectors include: tsetse flies, sand flies, tabanid flies, hippoboscid flies, fleas, bugs, and leeches. However, these parasites are much more adaptable to new environments than are the malarial parasites. This is proved by the ease with which most of them are cultured *in vitro*. Some of them grow and flourish on such media as potato juice, hydrolyzed haemoglobin, and some even grow on 3 per cent inulin. (Cleveland and Collier, 1930). Others are cultivated with difficulty, but it is still true that as a group they do grow *in vitro*, whereas such forms as rickettsiae, malaria, and piroplasmas have not yet been cultivated in the absence of living host cells. It is also true that the diversity of hosts of the haemoflagellates is not so great as it would seem at first when we reflect that all of the above-mentioned vectors are diptera except fleas, bugs, and leeches. Fleas are closely related in their evolution to flies. Of all the haemoflagellates known, the greatest majority of them are associated with diptera. The haemoflagellates of vertebrates may have evolved from two or three original sources,—one dipteran, one from hemiptera and another from leeches.

ORIGIN OF MALARIAL PARASITES

We shall consider under this heading the parasites of the blood belonging to the genera *Plasmodium*, *Haemoproteus*, and *Leucocytozoon*. Species of *Plasmodium* are found in the blood of man, monkeys, bats, birds and lizards and are transmitted by mosquitoes. They are the true malarial parasites, although this is a modified use of the term *malaria*, since it originally applied to the *disease* in man caused by species of *Plasmodium*. The malaria-like organisms belong to the genera *Haemoproteus* and *Leucocytozoon*. Species of the former occur very widely in birds and to a less extent in reptiles,—both turtles and snakes. Species of the latter are restricted to birds. Species of parasitic flies of the family Hippoboscidae are known to transmit *Haemoproteus* of birds. The vectors of reptilian species of *Haemoproteus* are unknown. Two species of *Leucocytozoon* are now known to be transmitted by blackflies or Simuliidae. (See O'Roke, 1930, and Skidmore, 1932.) The differences between these genera are not as marked or distinct as would seem upon superficial examination. They are generally considered to constitute two families, the Plasmodidae and the Haemoproteidae. The Plasmodidae contains the single genus, *Plasmodium*, and the Haemoproteidae the genera, *Haemoproteus* and *Leucocytozoon*. The distinction between the two families is usually stated to be that the asexual cycle occurs in the circulating blood in the case of the Plasmodidae, while this cycle occurs in "endothelial cells" in the case of the Haemoproteidae. We now know from the work of Huff and Bloom (1935) that in *Plasmodium elongatum* the asexual cycle may and does take place in cells other than circulating red

cells. James and Tate (1937) have also shown that the asexual stages of *Plasmodium gallinaceum* of the domestic fowl are definitely in cells of the spleen and other organs. It, therefore, seems probable that poor criteria have been used in setting up the two families, although it is likely that such natural groups do exist and that other criteria may eventually confirm the original separation. For example, the type of insect vector may prove to be a more valid criterion for separation of genera or families than the types of cells of the vertebrate invaded by the asexual stages. Since in all three of these genera the asexual cycle occurs in the vertebrate and the most of the sexual cycle in insects of the order Diptera, with no free-living stages in the life-cycles, the evolution of these forms assumes great interest and importance in the general problem under discussion.

One obviously has the choice of one of two main hypotheses in attempting to explain how the present state of complete, obligate parasitism may have arisen in this group. We may think of the parasites as having first become adapted to parasitism of vertebrates and then secondarily to the blood-sucking insects which ingested them along with the blood meal. The other possibility is to think of them as being parasites of insects first, probably before the latter had acquired the blood-sucking habit, and then as being transferred to vertebrates after the insect hosts became parasitic on vertebrates. There have been proponents of each of these hypotheses. Therefore, it is not with the claim to authorship of either of them that my views are given here, but rather with the desire of presenting evidence which seems very suggestive, if not conclusive, of the arthropod origin of the malarial parasites.

Regardless of the choice of one of these hypotheses, it would seem logical to assume that there ought to be today more of a parallelism between the parasites and the original group of hosts than between the parasites and the more recent hosts. In the case of such highly developed parasitism as the malarial parasites show, one must assume that the relationship between parasite and host is a very old one. We should, therefore, expect that the evolution of parasites and hosts should have paralleled each other. Likewise, we should expect a better adaptation to exist at present between the parasites and their first hosts than between them and their more recent hosts. This adaptation would result from the processes of natural selection, since the strains of parasites having the greatest pathogenicity for the host would be the most likely to be lost through the death of the host. Likewise the more susceptible of the hosts would be weeded out because of their greater likelihood of succumbing to the effects of the parasite. This type of evidence points in the direction of the insect origin of the malarial parasites.

Let us first consider the natural relationships of the two types of hosts—vertebrate and invertebrate. In the vertebrate hosts we find species of *Plasmodium* resembling each other very closely which parasitize man, monkeys, bats, birds, and lizards. If these parasites had followed the vertebrate hosts in their evolution it ought to follow that in the time it has required for the development of such divergent groups of vertebrates from common precursors the parasites should have changed enough that they would still not be placed in the same genus today. Likewise, there are closely similar species of *Haemoproteus* in present day birds and reptiles. One cannot retreat to the argument that parasites confined to the red cells of these animals would be protected

from the mechanisms of evolutionary change, since we know that the red cells of these various hosts are now widely different chemically and immunologically. The proponents of the theory of the origin of malarial parasites as occuring primarily in vertebrates, point to the close similarity of these forms to the intestinal coccidia. They believe that certain intestinal coccidia developed the ability to live in the blood and then lost the capacity for producing resistant oocysts and passing directly from vertebrate to vertebrate. There is little good evidence for or against such a theory. Such a transfer is known to occur in parasites of the family Lankesterellidae, sub-order Eimeriidea. The coccidian parasite, *Schellackia bolivari*, is found in the intestinal epithelium of the lizards, *Acanthodactylus vulgaris* and *Psammodromus hispanicus*, where it undergoes schizogony. There is a migration of the merozoites destined to produce gametocytes into the subepithelial connective tissue. Here the oocyst is formed and when it bursts, the liberated sporozoites enter erythrocytes and are taken up by the mite, *Liponyssus saurarum*. When the mite is eaten by the lizard, the sporozoites are liberated and they again enter the epithelial cells of the gut of the lizard. The adaptation is carried further in the genus *Lankesterella*. *L. minima* undergoes its complete development in the endothelial cells of the blood vessels of the frog and the sporozoites enter erythrocytes. They are then taken up and transmitted by a leech to other frogs. This is very suggestive of, but it cannot be taken as evidence in favor of, a similar happening in the Haemosporidiidea. In none of the species of the latter is there any evidence that epithelial cells are invaded. On the contrary, *Plasmodium elongatum*, which is not restricted to erythrocytes, lives in a large variety of host cells but these cells are all either blood cells or blood-forming cells (Huff and Bloom, 1935). The epithelial cells are not invaded.

The natural relationships of the invertebrate hosts of the Haemosporidiidea are closely parallel to those of the parasites themselves. Transmission of species of *Plasmodium* is by means of Culicidae or mosquitoes. Furthermore the species of *Plasmodium* in Primates are transmitted by mosquitoes of the tribe Anophelini, whereas those of birds are transmitted by species of the tribe Culicini. In other words, here is just the type of parallelism one would expect if the malarial parasites were primarily parasites of mosquitoes. In further support of this belief, the parasites are known to produce disease in the vertebrate hosts in many instances. Whereas it has been claimed that the parasites have some harmful effect on the mosquitoes, I have been unable to demonstrate any evidence in favor of it in my experiments with avian malaria and *Culex* mosquitoes. The stomach infections in much of my research have been massive. Yet when the infected and uninfected individuals were separately tabulated from the results of several years of research I failed to find any significant differences between the two groups in relation to (a) ability to lay viable eggs, (b) length of life following infective blood meal, (c) length of time between blood meal and oviposition, and (d) number of eggs laid following the blood meal.

Christophers (1934) supports the hypothesis of the insect origin of malaria on basis of the fact that the hosts of *Plasmodium*, birds, bats, and monkeys, live in trees where mosquitoes are more likely to bite them. He would also, in his study of the parasites of monkeys, apes, and man, interpret the absence of crescent forms in monkeys and their presence in

the great apes and man as giving support to the view that the latter constitute independent evolutionary stems, and he sees this possibility as suggesting Africa as the original home of malaria. In my opinion the absence of crescent forms in monkeys is, in addition to being negative evidence, not an important fact if we accept the mosquito origin of malaria. Both crescent and round gametocytes are found also in birds, and the crescent forms occur in reptiles. Therefore, I would think of this difference in fundamental type of gametocyte as having developed very early in the evolution of malarial parasites. It, perhaps, existed in the parasites of the common precursors of the anopheline and culicine mosquitoes. In that case, when mosquitoes began sucking blood from birds and mammals they passed on to both types of hosts species of malaria-like organisms already differentiated in the direction of producing different types of gametocytes. The transmission of the reptilian and chiropteran species of *Plasmodium* has not been worked out in any case as yet. It is a very important point to be determined in the establishment of this theory.

The known vectors of *Haemoproteus* are all Diptera Pupipara which are only distantly related to mosquitoes, but of course, these two groups belong to the same order and hence are more closely related to each other than the vertebrate hosts, birds and mammals. If the hypothesis of the insect origin of the malarias is valid, the vectors of the reptilian species of *Haemoproteus* should be sought for among the cyclorrhaphous blood-sucking Diptera.

The known vectors of *Leucocytozoon* are flies of the family Simuliidae. In this case, however, the vectors are more closely related to mosquitoes than to the hippoboscid flies which transmit *Haemoproteus*, whereas *Leucocytozoon* has been thought to be more closely related to *Haemoproteus* than to *Plasmodium*. However, this latter relationship is somewhat questionable and it may be that when the life-cycles of *Leucocytozoon* are better known they will be found to be closer to *Plasmodium* than has been supposed.

Some very interesting conclusions would follow upon the assumption that all malarial parasites were originally parasites of Diptera and were secondarily adapted to living in vertebrates. Since present day life-cycles of all of them involve schizogonous cycles in the vertebrate hosts it might be assumed that this is a later outgrowth from life within the vertebrate. In other words, the schizogonous cycle may have been intercalated into a like cycle essentially like that of the gregarines. Another interesting outgrowth of this assumption might occur. Since the life cycles of all three of the genera, *Plasmodium*, *Haemoproteus*, and *Leucocytozoon*, are practically identical in detail within their respective hosts, mosquitoes, hippoboscid flies, and blackflies, and since these diptera probably first transmitted the parasites to vertebrates through their bites, it would seem probable that the original cycle in the diptera involved passing the infective stage of the parasite through the mouthparts. This infective stage would have had to be a resistant stage which could live for a short time in the environment until it could be acquired by another similar fly. Another possible hypothesis would be that this infective stage was injected into and lived in plants before the insects acquired the blood-sucking habit. This would be in agreement with the general belief that the blood-sucking habit in insects arose from forms which were adapted to sucking plant juices. Another conclusion which would

result from the proof of these hypotheses is that although the stages of malarial parasites and piroplasmas which live in erythrocytes often resemble each other very closely, this similarity may have developed recently because of long residence in similar environments. Actually the two groups would appear on this hypothesis to have had widely divergent courses of evolution.

It is unfortunate that fossil records are too inadequate to give us any positive evidence for or against this theory. However the following statement about the geological history of the Culicidae by Edwards (1932) is of interest in this connection:

> Since we have reason for believing that the order Diptera arose not later than the Triassic Period, and since the *Culicidae* are certainly one of the more primitive families of the order, it is highly probable that members of this family existed during the Jurassic period, before the age of mammals; the fact that many *Culex* at the present day attack lizards and frogs suggests that even the blood-sucking habit may have been developed at this early period. Unfortunately the known insect-bearing beds of Jurassic or Cretaceous age are few, and in them no remains of *Culicidae* have yet been found. We have therefore no direct palaeontological evidence as to the time of origin or phylogenetic history of the family. In the Oligocene rocks of the Isle of Wight and Germany remains of *Culicidae* are numerous, but the species hardly differ from those of the present day; all the three subfamilies are represented, as well as the genera *Dixa*, *Chaoborus*, *Mochlonyx*, *Culex*, *Aëdes* and perhaps *Theobaldia*, *Mansonia* and *Megarhinus*. Our knowledge of fossil *Culicidae* was reviewed by the writer in 1923 (Quart. Journ. Geol. Soc., 79, p. 139, 155)."

There remains one point to be stressed. If we assume for the moment that the theory that malarial organisms began their parasitic existence in vertebrates is the correct explanation, we must assume that the ability to transmit these parasites was more or less accidentally acquired by arthropods which took up the habit of sucking blood from these vertebrates. It then becomes difficult to understand why the only successful vectors were diptera. Since such forms as lice, fleas, ticks, and mites have been parasitic much longer than most of the diptera it would seem that some of them had an excellent opportunity to establish a harmonious relationship with the parasites. Yet this does not seem to have happened.

SUMMARY AND CONCLUSIONS

Since little or nothing is known of the early evolutionary development of most of the organisms producing disease and since the possibility of ever securing much information bearing on this question seems remote, an attempt has been made here to approach this question from a study of contemporary parasites and their relations to their hosts. The organisms chosen are all arthropod-borne and belong to the rickettsiae, the spirochaetes, and the protozoa. In general they represent parasitism of a high degree in which there are no longer any free-living stages. The evidence seems to be strongly in favor of the belief that parasitism among these groups began in the invertebrates and was secondarily transferred to vertebrates when the invertebrate hosts became blood-sucking. This seems to hold for the rickettsiae, certain spirochaetes, the piroplasmas, the haemoflagellates, and the malarial organisms. Extension of the theory to include other groups, or generalizing upon the cases here presented should be done with due caution and consideration for the relationships within the other groups. It is hoped that the method employed here will be extended by others to the parasites in which they are most interested. The problem would seem to be important enough to warrant the collection of facts and the planning of experiments to clarify some of the points now poorly understood.

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THE PUBLISHING BEHAVIOR OF BIOLOGISTS

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I

THE index of the *Review of Applied Mycology* for 1935 lists 1085 authors (including joint authors) who published one paper dealing with plant pathology; 285, 95, 31, 24, 5, 3, 1. Other authors published respectively 2, 3, 4, 5, 6, 7 or 8 papers.

For that year (1935) the observed frequencies (*o*) of authors communicating 1, 3, 4 papers do not deviate significantly from the frequencies calculated (*c*) by assuming that the frequency for one paper being $(3.20)^6$, that for 2, 3, 4, 5 is respectively $(3.20)^5$, $(3.20)^4$, $(3.20)^3$, $(3.20)^2$, 3.20 (Table 1 and Graph 1).

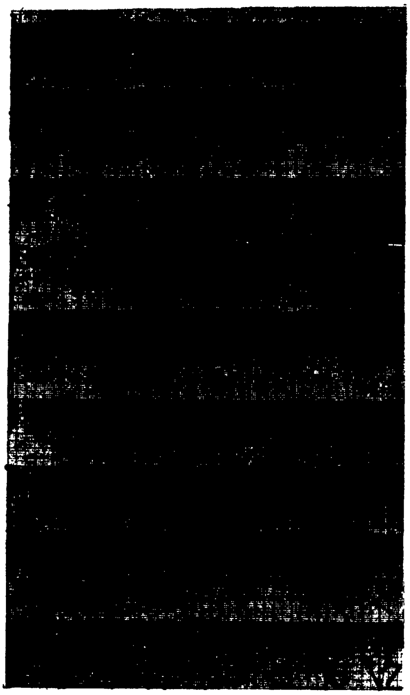
In other words, the assumption is that the probability for an author to publish one paper being p_1, that to publish 2, 3, 4, 5 is respectively $(p_1)^2$, $(p_1)^3$, $(p_1)^4$, $(p_1)^5$ which is one example of application of the general law of compound probability.

The observed frequencies for two papers are consistently significantly lower than

TABLE 1

Frequencies (o) of authors having 1, 2 [illegible] papers abstracted in the Review of Applied Mycology

Year	x	o		c	(o − c)	(o − c)²	(o − c)²/c
1932	1	951		887	64	4096	4.6
	2	224		286	62	3844	13
	3	82		92	10	100	1
	4	27		29.8	2.8	7.8	0.27
	5	22		9.6	12.4	153	16
	6	13		3.1	9.9	-	-
							34.87
1934	1	1032		1033.6	1.60	2.56	0.0002
	2	244		325.18	81.	6561	20
	3	101		102.26	2.26	5	0.05
	4	31		32.15	1.15	1.32	0.04
	5	17		10.11	7	49	4.90
	6	8		3.18	4.8	23.	7
							31.99
1935	1	1085	$(3.20)^6$	1073.7	13.3	177	0.165
	2	285	$(3.20)^5$	335.5	50.5	2550	7.40
	3	96	$(3.20)^4$	104.86	8.86	77.44	0.73
	4	31	$(3.20)^3$	32.77	1.7	12.90	0.10
	5	21	$(3.20)^2$	10.24	10.8	116	11.37
	6	5	(3.20)	3.20	1.8	3.24	1
							20.76



GRAPH 1. THE LOG OF CALCULATED FREQUENCIES OF AUTHORS, $3.1F^{(y-x)}$ ARE PLOTTED AGAINST THE NUMBER (x) OF PAPERS PUBLISHED IN THE YEAR, RESULTING IN A STRAIGHT LINE; THE OBSERVED FREQUENCIES y FOR x = 1, 2, 3, 4 PAPERS DO NOT DEVIATE SIGNIFICANTLY FROM THE STRAIGHT LINE

the calculated frequencies, while the observed frequencies for more than four papers are markedly in excess, suggesting that authors who contribute two and

TABLE 2

Frequencies (o) of authors contributing 1, 2, 3, 4 papers to vol. 120 of Comptes rendus Société de Biologie

$$\frac{o_x}{o_x + 1} = n = 4.25$$

x	o	c	$o - c$	$(o - c)^2$	$\frac{(o-c)^2}{c}$
1	330	326.26	3.75	14	0.04
2	69	76.76	7.76	60	0.80
3	14	18.06	4.06	16	0.80
4	5	4.25	1.75	3	0.70
					$2.34 = \chi^2$

those who publish more than four papers constitute a group which does not belong to the same series with authors contributing one, three or four papers.

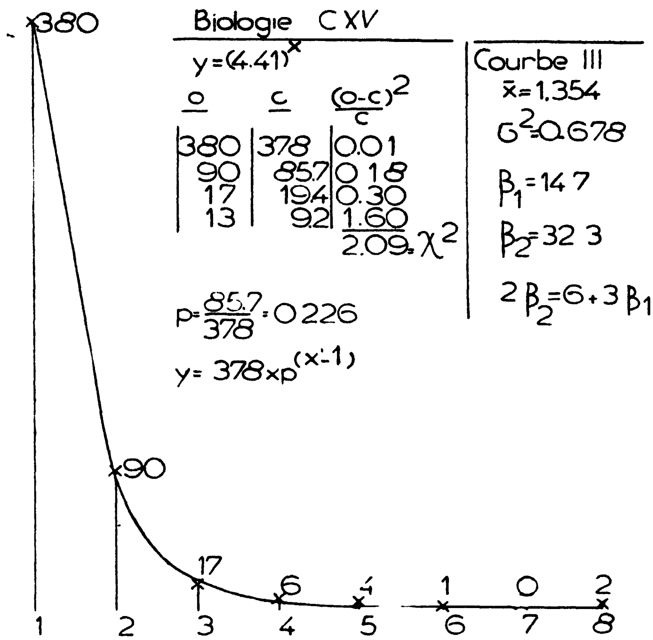
For the three years 1935, 1934, 1932, the observed frequencies for 1, 3, 4 papers do not differ significantly from the calculated frequencies, χ^2 being less than 5.99 which is the χ^2 value for $P = 0.05$ for 2 degrees of freedom.

x	1932	1934	1935	χ^2
1	4.6	0.0002	0.165	4.8
2	13	20.	7.40	40.40
3	1.	0.05	0.70	1.75
4	0.27	0.04	0.10	0.51
5	16.	4.90	11.3	32.

TABLE 3

Frequencies (o) of authors having had $x = 1$, $x = 2$ .... $x = 5$ notes printed in the Comptes rendus Société de Biologie, v. 115, or v. 118, during the period of January–April 1934 or 1935, respectively

	VOL. 115			VOL. 118		
x	o	c	$\frac{(o-c)^2}{c}$	o	c	$\frac{(o-c)^2}{c}$
1	380	$4.4^4 = 372.5$	0.15	330	$4.3^4 = 311.2$	0.37
2	90	$4.4^3 = 85.2$	0.24	81	$4.3^3 = 79.5$	0.026
3	17	$4.4^2 = 19.3$	0.27	23	$4.3^2 = 18.5$	1.1
4	6	$4.4^1 = 4.4$	0.60	2	$4.3^1 = 4.3$	1
5	4	$4.4^{\frac{1}{2}} = 2.1$	0.70	1	$4.3^0 = 1^{5.3}$	
6	1^5	$4.4^{\frac{1}{4}} = 1.4^{3.5}$				
7	—					
8	2					
			1.96 χ^2			2.496 χ^2



GRAPH 2. THE FREQUENCIES OF AUTHORS ARE PLOTTED AS ORDINATES (y) AGAINST THE NUMBER OF PAPERS (x) CONTRIBUTED TO VOL. 115 OF COMPTES RENDUS SOCIÉTÉ DE BIOLOGIE

The distribution can be adjusted to the equation $y = (4.41)^x$, or to a type III Pearson curve assuming the mean number of papers being $\bar{x} = 1.354$.

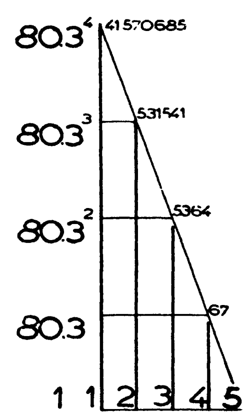
II

The distribution of frequencies of authors (or joint authors) from various parts of the world communicating 1, 2, 3, 4 papers published in vol. 120 of *Comptes rendus de la Société de Biologie* (1935) so approximates the distribution of $(4.25)^x$ that each individual value of χ^2 (for each discrepancy) is very probable, while the 4 values sum up to 2.34 which is very close to 2.36, the most probable value for 3 degrees of freedom. (Table 2.)

Barring the erratic behavior of a few individuals contributing more than 7 papers a year, the distribution of frequencies of authors contributing 1 to 7 papers a year can be adjusted to a simple calculated distribution. (Table 3.)

For two degrees of freedom, the probabilities of $\chi^2 = 1.96$ and $\chi^2 = 2.5$ are about $P = 0.40$ and $P = 0.30$. The calculated frequencies therefore do not differ significantly from the observed.

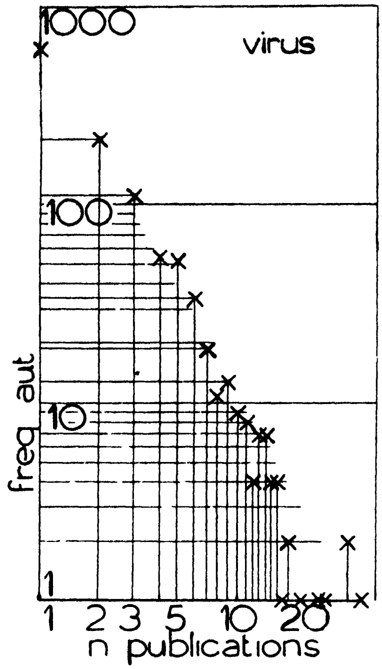
Examples 1 and 2 illustrate the general



GRAPH 3. A TOTAL OF 42,107,657 BIRTHS WERE RECORDED IN GERMANY FOR 1901–1935: THE OBSERVED FREQUENCIES (y^1) OF SINGLE BIRTHS, OF TWINS, TRIPLETS OR QUADRUPLETS ARE LISTED BELOW AND CHECKED AGAINST THE FREQUENCIES CALCULATED FROM $80.3^{(5-x)}$ WHERE x IS THE DEGREE OF MULTIPLE BIRTHS

Degree (x)	*Observed* (y^1)	*Calculated* (y)	
1	41,570,685	41,577,864	$= 80.3^4$
2	531,541	517,781	$= 80.3^3$
3	5,364	6,448	$= 80.3^2$
4	67	80.3	
5	0	1	

On the graph, the log of the frequencies are plotted as ordinates against the "degrees" of multiple births.



GRAPH 4. NUMBER OF AUTHORS (ORDINATES) HAVING HAD 1, 2, $\cdots$ n PAPERS LISTED IN M. T. COOK'S PARTIAL BIBLIOGRAPHY ON PLANT VIRUSES
(Plotted on log. grid.)

law: if the separate probability of each of n several independent events be p the probability of all n events occurring is p^n.

Again the formula $n^{\frac{1}{x-1}}$ is that presented by German statisticians to express the frequency relation of multiple births; n is the ratio of single births to twin births that occur in a given series of birth statistics [in a series quoted by the *Journal of Heredity* (26: 256, 1935), $n = \frac{41,570,685}{531,541}$] while x shows the degree of multiple births in question (i.e. twins, triplets, quadruplets). (Graph 3.)

M. T. Cook listed 3214 publications about plant viruses, originating from 1161 authors, 700 of whom contributed 1 paper, 110 contributed 2; the frequencies (y) of authors contributing 1, 2, 3, 4, $\cdots$ x papers can be plotted on a straight line using equation $\log(y) = -k \log(x)$.

NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that The Quarterly Review of Biology *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* The Quarterly Review of Biology, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

Genetics and the Origin of Species.
By Theodosius Dobzhansky. Columbia University Press, New York. $3.60. 9 x 6; xvi + 364; 1937.

When Darwin propounded his theory of the origin of species by the natural selection of heritable variations little was known in detail of two of the phenomena on which the theory was based—the origin of new variations and their perpetuation by heredity. Since then the geneticists have added much to our knowledge of these phenomena. For a time, indeed, it seemed that this new knowledge did not contribute much to our understanding of the origin of species. Later developments in genetics, however, have thrown more light on the problem and the purpose of this book is to synthesize this information.

Changes in the chromosomes—mutations of individual genes, rearrangements of the genes within the chromosomes, and reduplications and losses of whole chromosome sets (polyploidy)—furnish the raw materials for evolution. In different environments natural selection will favor different combinations of genes and thus separate what was a continuously varying population into subgroups with discontinuities between them. Besides this effect of selection in producing adaptive discontinuities there is another non-adaptive factor also tending to split a species into subgroups. As Hardy has shown, in a large population unaffected by mutation or selection the relative frequencies of various genes remain constant from one generation to another. In small populations, however, deviations from the expected frequency of a gene occur and once the frequency of a gene reaches zero the gene is irretrievably lost unless it is reintroduced by a new mutation. The effective size of population, moreover, is not the total number of the species but the size of the colony within which interbreeding occurs. Thus in different colonies chance factors will eliminate different genes and produce local races. It is probable that the local races of *Partula* which Crampton found in the different valleys of Moorea and for which he could find no adaptive explanation were produced by this chance mechanism.

The isolation of such local races is, however, not absolute and interbreeding between them would in time break down the discontinuities which separate them. A number of mechanisms prevent such interbreeding. The races may breed at different times. Members of a different race may be less attractive sexually than members of the same race. Finally different mutations and rearrangements of the genes in the different groups may prevent interbreeding or render the offspring sterile. Such isolating mechanisms fix

the diversity which has been produced by mutation, selection and random variation in frequency of genes.

This is a book which will repay careful study by every biologist. It includes a bibliography of 28 pages and an index.

EVOLUTION AND ITS MODERN CRITICS.
By A. Morley Davies. Thomas Murby and Co., London. 7s. 6d. net. $7\frac{3}{8} \times 4\frac{7}{8}$; xii + 277; 1937.

The original plan was to publish this work primarily as a reply to Mr. Douglas Dewar's "Difficulties of the Evolution Theory" but the book has been expanded beyond that point so as to serve as an expression of the author's own ideas on evolution rather than as a mere rejoinder. After a survey of old and new ideas of evolution, sample families selected from the Mammalia and Mollusca, with detailed discussions of the paleontological records, are introduced to test "evolution within the family but not beyond it." The formidable assortment of facts and pseudo-facts that have been accumulated by a long line of biologists in support of various principles of evolution is subjected to an analysis that is as brilliant as it is comprehensive. There is a glossary, a short bibliography, and an index.

LIFE LONG AGO. *A Story of Fossils.*
By Carroll L. Fenton. Reynal and Hitchcock, New York. $3.50. $9\frac{1}{4} \times 6\frac{3}{4}$; x + 287 + 14 plates; 1937.

The youngster who has a scientific bent strong enough to keep him from being phased by a whole set of new and usually long words, may derive considerable pleasure and quite likely a zest for some fossil hunting from this story of the fossil remains of prehistoric life. The author takes his young readers with him on imaginary excursions to regions where the now fossilized plant and animal forms once abounded as living things. Here he reconstructs for his audience the story of this long ago existence and shows how, from a study of fossils of different ages, changes in form may be seen taking place. Fourteen plates of photographs and many drawings of fossils and "restorations" are included to illustrate the text.

A CHALLENGE TO EVOLUTIONISTS.
By Douglas Dewar. Thynne and Co., London. 2s. 6d. $7\frac{7}{8} \times 5\frac{1}{4}$; 63; 1937.

This is a report of Mr. Dewar's remarks on evolution, made during a debate with Mr. Joseph McCabe on February, 1937. Since Mr. McCabe's speeches have not been included, the author regards the volume in the nature of a challenge to evolutionists.

GENETICS

YEARBOOK OF AGRICULTURE, 1937.
U. S. Department of Agriculture. Government Printing Office, Washington. $2.00. $9 \times 5\frac{3}{4}$; 1497; 1937.

AGRICULTURAL STATISTICS, 1937.
U. S. Department of Agriculture. Government Printing Office, Washington. 50 cents. $9\frac{1}{4} \times 5\frac{3}{4}$; 486; 1937 (paper).

The present volume of the yearbook of agriculture brings to completion the reports of a committee which set out in 1933 to make a survey of practical breeding and genetic research with those plants and animals that are of importance in American farming. The report on major crop plants and classes of live stock were published in the 1936 yearbook. The series of papers presented in this number cover a variety of subjects: garden vegetables, northern tree and bush fruits, subtropical fruits, flowers, nut trees, forest trees, forage grasses and legumes, Angora and milk goats, turkeys, ducks, furbearing animals, honeybees, and the dog. The general purpose underlying this survey of breeding and genetics has been an attempt to show the achievements of the past, the situation of the present, and the possibilities of the future. The yearbook is as usual prefaced by the yearly report of the Secretary of Agriculture to the President.

In the 1937 volume of Agricultural Statistics, all of the most important statistical data of the United States, and

of the world so far as they relate to the agriculture of this country, are brought together and presented in some 574 tables. An index makes possible easy reference to all data.

GENERAL BIOLOGY

DESIGN IN NATURE.

By James Ritchie. Charles Scribner's Sons, New York; County Life Limited, London. \$2.00. $7\frac{1}{4} \times 4\frac{7}{8}$; 142 + 29 plates; 1937.

This book might well have been entitled "Rhythms in Nature," because the author has seized every opportunity available to show that all life, whether simple or complex, follows a definite cycle or rhythm in all of its manifestations. Mr. Ritchie has shown that the foremost of all rhythms or cycles in the living world is the energy cycle which includes: (1) the capture of energy from the sun by green plants and the conversion of that energy into a form available to animals; (2) the use of the energy by animals; and (3) the combination of a portion of it with more energy from the sun into more green plants. The cycles of day and night and of winter and summer are so important in their influences that the result is a constantly moving cycle of activity and inactivity among almost all forms of life.

The volume is clearly written in a stimulating style, and is rich in first hand observations drawn from the author's personal experience as a naturalist in the field. It is not difficult for us to understand how a man so intimate with nature as the author could attribute song (frog's, bird's, etc.,) to a sense of well-being in the animal. However, to our knowledge, no scientific proof has yet appeared to show that this is the case, and until it does, it requires a slight stretch of imagination to attribute to either frogs or birds the intelligence required to reason, " 'God's in his heaven; all's right with the world'—I'll sing."

The book is intended not so much for the student of natural history, as for the layman who has an earnest desire to learn more of the intricacies of life about him. In addition to a short table of contents and an index, the text contains a large number of illustrations that would greatly enhance the value of any study in natural history.

GENERAL SCIENCE: BIOLOGY.

By E. R. Spratt and A. V. Spratt. University Tutorial Press, London. 2s. 6d. $7\frac{1}{4} \times 4\frac{3}{4}$; vii + 216; 1937.

A TEXTBOOK OF GENERAL BIOLOGY. *Second Revised Edition.*

By E. Grace White. C. V. Mosby Co., St. Louis. \$3.00. $8\frac{1}{2} \times 5\frac{1}{2}$; 667; 1937.

The first of these books is one of the best elementary books in general biology that we have seen in many a day. It was prepared with the idea of presenting the essentials of biology to those wishing to take various English school certificate, Civil Service or L.C.C. clerkship examinations. The types are well selected and easy to obtain. The diagrammatic method of illustration is largely used and much thought and skill has gone into the preparation of these drawings. Some of the main problems of food and health of the human race are discussed briefly and simply. There is an index.

The second book listed (a college text) has, under the able editorship of its author, maintained the high quality of the first edition (noticed in Q.R.B. Vol. 8, No. 4) As in the earlier issue the assistance of outstanding biologists has been obtained in developing the various sections. In the revision there have been embodied many suggestions of those who have used the book. The subject matter has been rearranged, new material included and the number of illustrations, which are excellent, increased to 336. A glossary and index complete the volume.

ASCARIS: *The Biologist's Story of Life.*

By Richard Goldschmidt. Prentice-Hall, New York. \$3.25. $8 \times 5\frac{1}{2}$; ix + 390; 1937.

"Man must be taught as if you taught them not . . . ," and in its charmingly conversational style this book written for the layman's biological edification

adheres closely to the 18th century aphorism. The story opens with the roundworm, *Ascaris*, enjoying an exceedingly leisurely parasitic existence in a horse's small intestine. However, he is, in short order, rudely removed by the author from this comfortably alkaline position, killed with ether fumes, and dissected. Using *Ascaris* as a constant focal point, the writer digresses into descriptions of divers animals, their adaptations to the environment, and the interrelations of all life including man. Animal forms, regeneration, transplantation, and inheritance of acquired characters are discussed in the first few chapters; the succeeding chapters likewise touch many fields of physiology, histology, bacteriology, immunology, evolution, and genetics in kaleidoscopic and interesting review. The use of non-scientific language and splendid illustrations serve to make this a charming story of the wonders of the biological world with a roundworm as its hero and the story of his life as a source of all biological revelations.

BIOLOGY FOR MEDICAL STUDENTS. *Second Edition.*

By C. C. Hentschel and W. R. Ivimey Cook. With a Foreword by G. E. Gask. Longmans, Green and Co., New York. \$7.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; xii + 664; 1937.

In view of the revised examination syllabuses of several English universities, this text-book has in turn been revised and new material has been added. The first half of the work is devoted to the animal kingdom, the material being presented by the study of a particular form in each of the larger phyla. The vertebrates are treated by classes, including a chapter on embryology.

Dr. Cook's section on the plant kingdom deals primarily with the flowering pant, including anatomy, histology, and physiology. In the remainder of the book plants, animals, and bacteria are considered in their adaptation to the environment—particulary those forms which are parasitic to the human host.

The text is admirably written and organized, but would require accompaniment by an extensive laboratory course to insure clear understanding by the novice. Those of the illustrations which are uncopied are frequently poor in quality. The table of pefixes and suffixes should be of help to the student.

WEATHER. *Natural History Studies.*

By Gayle Pickwell. Hugh F. Newman and Co., Los Angeles. \$3.00. $11\frac{1}{2}$ x 9; x + 170; 1937.

There is probably no single factor in man's environment that affects him more than the weather. It affects him not only physiologically but economically and socially as well. And it is impossible to get away from the weather. It may be good or it may be bad, but it is always something. If we accept Huxley's definition of a liberal education we must believe that it is incumbent for anyone who wishes to consider himself liberally educated to acquire at least a rudimentary knowledge of meteorology. Yet the average man's knowledge of meteorology consists of a few superstitions connected with rainbows and groundhogs. To all such people the present work may be most highly recommended. It is an excellent example of modern bookmaking with photographs running to the edges of the page, good paper, and clear type. There are illustrations of thirteen types of clouds, three types of fog, several kinds of lightning, of different kinds of landscapes, with descriptions of the kind of climatic conditions that produce them. There are also descriptions of various kinds of storms, the work of wind and water, and instructions as how to read weather maps.

The book is one of a series of introductory works in natural history, bound together by the photographs, all of them the work of the same author.

THE RÔLE OF CHEMIOTAXIS IN BONE GROWTH.

By A. P. Bertwistle. Henry Kimpton, London. 8s. 6d. net. $9\frac{3}{4}$ x 6; xii + 59; 1937.

"That bone is the most interesting tissue

in the body is," in the opinion of this author "beyond dispute." The purpose of his book "is to describe Disruptive Chemiotaxis, a new process to medicine; and to lay down a law which, based on histological grounds will be found to cover the whole field of bone pathology, viz.: "*That whenever young fibrous tissue, particularly young blood-vessels, come into contact with bone or a calcified deposit, new bone formation occurs.*" Disruptive chemiotaxis is "the power of certain hard substances of attracting and drawing into themselves certain soft, living structures." One of his prime illustrations of this tropism is that the roots of germinating seeds pierce the hard ground instead of lifting the seed in the air, which does not seem to us to bear much relation to what he later describes as taking place in bone diseases. Most of the text consists of briefly cited examples from pathology to prove this all-inclusive theory.

An Experimental Study of the Problem of Mitogenetic Radiation. *Bulletin of the National Research Council, Number 100.*

By Alexander Hollaender and Walter D. Claus. National Research Council, Washington, D. C. $1.00. $9\frac{3}{4}$ x $6\frac{3}{4}$; 96; 1937 (paper).

The purpose of this investigation was to prove or to disprove the existence of the so-called mitogenetic rays. Both biological and physical detectors were used in a series of carefully carried out experiments and in all cases there was no indication that any measurable ultraviolet radiation was given up by typical mitogenetic senders. As a biological sender, colonies of *Escherichia coli* were used, while a Geiger-Müller photon counter was the physical detector.

The problem was further investigated by determining the sensitivity of biological materials to ultraviolet radiation in the range of intensities which approach those reported for mitogenetic rays. Some effects on growth properties of surviving organisms were noted, but the energies needed to produce these effects were outside the limits of the energies reported for mitogenetic radiation.

The Physical Basis of Geography. *An Outline of Geomorphology.*

By S. W. Wooldridge and R. S. Morgan. Longmans, Green and Co., New York. $4.80. $8\frac{1}{2}$ x $5\frac{1}{2}$; xxi + 445; 1937.

In the first 10 chapters of this textbook the authors present an outline of geophysics and structural geology including considerations of the facts and theories regarding the origin of the earth, the constitution of its interior, isostasy, the nature and origin of earth-movements, the building of mountains, vulcanicity and formation of rocks. In the final 13 chapters, land-forms due to erosion are discussed at length. In the exposition of the subjects treated in these chapters the authors follow the methods and ideas laid down by Davis. While this is primarily a textbook for students of geography it is written in such an interesting fashion that it should find favor with ecologists generally. The photographs and illustrations are clear and helpful.

This Is Our World.

By Paul B. Sears. University of Oklahoma Press, Norman. $2.50. $7\frac{3}{4}$ x $5\frac{1}{4}$; xi + 292; 1937.

Books written for noble and admittedly worthy puposes are highly irritating to most of us. Even our most righteous citizens find that once a week is enough for sermons. We are therefore pleased to report that straight through to page 233 this treatise on soil and water conservation is most agreeable and interesting reading. Mr. Sear's literary style, buttressed by comic cartoons, is such as to coax rather than bully us into agreeing with him. But then, having won his point, he proceeds to tack on an irrelevant section about sociology and ends with a Fourth of July political speech which succeeds in taking all the healthy sting from what he has previously said.

La Vie Cellulaire Hors de l'Organisme. *La Culture des Tissus.*

By Jean Verne. G. Doin and Cie, Paris. 38 francs. $8\frac{5}{8}$ x 6; x + 192; 1937 (paper).

The author has not attempted to write a complete treatise on tissue culture, but has selected interesting aspects of the subject for brief discussions. Cells *in vitro* possess a morphology and a physiology that contribute important data to biological problems, and have led to significant results. Removing cells from the influence of the organism discloses potentialities which otherwise do not usually appear. This permits new approaches to the study of normal and pathological reactions and it is this aspect of the subject that the author dwells on.

AIDS TO THE STUDENT OF CONSERVATION. *Review and thought-provoking questions concerning Our Natural Resources and Their Conservation. Edited by Parkins and Whitaker.*

By Stephen S. Visher. John Wiley and Sons, New York; Chapman and Hall, London. 25 cents. $10\frac{3}{4} \times 8\frac{1}{4}$; 32; 1937 (paper).

This pamphlet was prepared to accompany Parkins' and Whitaker's *Our Natural Resources and Their Conservation* which was published in October 1936. For each of the twenty-three chapters of the original, Dr. Visher has supplied a page of "review" and "thought-provoking" questions. Spaces for brief written answers are provided for many of the questions and the pages are perforated so that the separate tests may be easily torn out and handed in to the instructor.

HUMAN BIOLOGY

COLLECTED STUDIES ON THE DIONNE QUINTUPLETS. *St. George's School for Child Study, The University of Toronto.*

By W. E. Blatz, M. Chant, M. W. Charles, M. I. Fletcher, N. H. C. Ford, A. L. Harris, J. W. MacArthur, M. Mason and D. A. Millichamp. University of Toronto Press, Toronto. $4.00. 9×6; [8] + 206 + 39 plates + 13 tables + 9 charts + 5 folding charts; 1937.

This book consists of six different papers reporting investigations carried out on the Dionne quintuplets under the direction of St. George's School for Child Study of the University of Toronto.

In the first paper J. W. MacArthur and N. H. C. Ford discuss the evidence for monozygosity in these siblings. On the basis of the nature of the fetal membranes, the close resemblance of the quintuplets in a great number of hereditary characters, and the youth of the mother coupled with the absence of any inherited proclivity to production of twins in the immediate family they conclude that the set is derived from a single ovum. The medical literature on quintuplet births is discussed and a pertinent bibliography appended. Eight plates are presented, including finger, palm and sole prints of all the children, and right and left profile and full face views.

The second paper by W. E. Blatz and D. A. Millichamp presents the results of mental tests begun in the 11th month of life and continued at intervals of two months. Three test forms were used, the Gesell, the Kuhlman and the Merrill-Palmer (Stutsman). The actual test results are shown in an appendix. The apparent retardation of these five children is attributed primarily to their prematurity, with two other factors difficult of evaluation, environmental arrangements and inheritance, postulated as contributory.

The third paper on the early development of the quintuplets, by W. E. Blatz, D. A. Millichamp, and M. W. Charles presents an analysis of observations from the 12th to the 36th month. Records were made of the social contacts of each child with the other four. The results are presented in detail in eight tables and 7 charts. The conclusion is reached that "these five children already manifest quantitative and qualitative social and personality differences of a more or less stable nature."

The fourth paper by W. E. Blatz, D. A. Millichamp, and N. Chant, presents data pointing to the conclusion that the five children show marked individual differences in their emotional background and in their attitudes towards authority. It is suggested that "however one defines personality the chief influences towards

its development are environmental rather than hereditary."

The fifth paper by W. E. Blatz, D. A. Millichamp and A. L. Harris, describes in considerable detail the sleeping, eating, washing, dressing, play and elimination routine of the children, and the way problems which arose were solved. Sample charts and records are presented.

The sixth paper on early development in spoken language of the quintuplets, by W. E. Blatz, M. I. Fletcher and M. Mason presents data collected from the 12th month. Every new sound which each child made was recorded by the staff in charge. A complete chart for each child is contained in an envelope on the back cover. The quintuplets were found to be slow in starting to use syllables and words, as compared with a control group of 13 children.

Many photographs of the quintuplets at various ages, and their environment are included.

AMERICA'S YESTERDAY.

By F. Martin Brown. J. B. Lippincott Co., Philadelphia. $3.50. 9 x 6; 319 + 32 plates; 1937.

The science of archaeology has never been adequately synthesized. This is particularly true of American archaeology. Except for a work by Alphaeus Hyatt Verrill that did not enjoy the popularity it deserved, this is probably the first work of the kind to appear. That it was badly needed is shown by the surprising conclusions reached by combining the findings of separate investigators.

The author believes that man has been in America a much longer time than commonly supposed. On the basis of the Folsom flints, the association of the bones of the Natchez man with those of the ground sloths, and the occurrence of human bones in the glacial drift of Lake Agassiz, he concludes that man appeared in America during the first interglacial. This primitive American was in no way essentially different from *Homo sapiens* but *Homo sapiens* did not arrive in Europe until the third interglacial. All older Europeans were Neanderthal men, a type not known in America. We must therefore conclude either that the geologists who correlated the glacial gravels of America and Europe have widely missed the mark, which seems highly improbable in view of the quantity of study that has been done on this subject, or that *Homo sapiens* has been established in Europe since the Chellean, or that America was inhabited by a race of submen ancestral to *Homo sapiens*. No empirical evidence in support of either of these latter contingencies has ever been discovered.

Concerning the origin of American civilization the author takes a more conservative view. He agrees with most other anthropologists that American civilization is indigenous, as evidenced by the lack of the domestic animals and plants of the old world prior to the coming of the conquistadores. The age of American civilization he estimates at 4000 years, though conceding that this figure may have to be revised when the age of the pedregal at Cuicuilco is known. He seems ignorant of the study of the pedregal made by Mena and Hyde, who put its age at 5000 years, that of the loess below it at 2000, and the cultural age of the artifacts from beneath the loess at about 1000, making 8000 altogether. Also he ignores the excavations carried on by the University of Arizona, which disclosed that the pyramid of Cuicuilco is only the last of four placed in cone-in-cone formation.

It is obviously impossible to include in a book of this size everything that every reader will wish to know about, and many will be disappointed to find no mention of the artifacts from the Delaware gravels found by Xantus in the fifties of the last century, or of the Lenape Stone with its engraving of the two hunters attacking a mastodon. Also, many readers will wish to question the statement that of the four native governments developed on this soil that of the Incas was communistic, that of the Mayas oligarchic, that of the Aztecs autocratic, and that of the Pueblos democratic. This is important if true, for the democratic Pueblos alone survived the conquest. But since the Maya empire disappeared before the Spaniards arrived, and since the Spaniards made every effort

to eradicate the memory of their past glory from the minds of the natives by burning their books and burying their pyramids, it is difficult to see how such definite appraisals of these political systems can be made. At least one authority, Hewitt, has denied that the Aztecs were autocratic.

These criticisms are not intended to be derogatory—the point is that this is the kind of book that makes the reader think. No one interested in American prehistory can afford not to read it. It is well written and the illustrations are excellent, only there are not enough of them. There is a complete index, two chronological tables, a bibliography, and a chapter on tree-ring dating.

Caste and Class in a Southern Town. *By John Dollard. Institute of Human Relations, Yale University Press, New Haven; Oxford University Press, London.* $3.50. 9 x 6; vi + 502; 1937.

The research site was an unnamed Southern town having a population slightly in excess of 2500; and the information was assembled by means of interviews that took place in an office maintained especially for that purpose by the author, in the homes of the informants, or at social gatherings of one sort or another. Since the object was to obtain a fairly representative cross-section of both white and Negro opinion, it necessarily follows that the writer had to remain on good terms (while in a southern town) with members of both races.

The bias of the various informants is readily admitted at the outset. Many of the whites were suspicious of the author's motives, yet were, in varying degrees, anxious that their side of the case be adequately presented; the Negroes, on the other hand, were often aware of the possibilities of favorable publicity for their cause that might come from any such sociological investigation.

In the chapter on "Bias" the author has inserted the following as a footnote: "The writings and views of Freud have become so thoroughly worked through my thikning that I had rather ascribe to him a major orientation of my thought than cite him as frequently as I would otherwise have to."

There are chapters on caste and class; caste patterning of education; caste patterning of politics; accommodation attitudes of Negroes; aggression within the Negro group; Negro aggression against whites; white caste aggression, etc.

Probably what is brought out most clearly is the extraordinary pressure that the whites bring to bear on the Negroes in order to make them conform to certain behavior patterns. At this point it might be noted that statistics on lynching are not as significant as reformers would have us believe, for the Southern whites can and do make the Negroes conform without resorting to that extreme measure. The author shows that it is the white middle class, and not the poor whites, who are more concerned with keeping the Negro in his place. Negroes have one effective weapon against the whites, and they make use of it frequently. They clear out without giving notice, and often during the busy season. The fact that crimes of violence are numerous in the Negro quarter is partially attributed to the total lack of opportunity that any Negro has to repay a white in kind, with the consequence that he has to vent his animosities on members of his own race. Considerable point is made of the amount of sexual contact that is presumed to take place between white men and Negro women. The writer claims that one of the reasons why the whites are so harsh with Negroes is because they (the whites) fear that Negro men, given the power, would hand out to white women the same treatment that Negro women are now receiving from white men.

A few remarks on the methodology used in the study would not be out of place. While it is obvious to any intelligent reader that the race imbroglio in the South has its psychopathic aspects, and that a psycho-analytic approach could be employed to great advantage, obviously great caution must be exercised in accepting the remarks of unidentified informants, particularly on such subjects as sexual behavior, as constituting a body of facts to be used in interpreting the mores of a community.

SAVAGE CIVILIZATION.

By Tom Harrisson. Alfred A. Knopf, New York. \$4.00. $8\frac{3}{4} \times 5\frac{3}{4}$; 461 + 32 plates + 1 folding chart; 1937.

When the Oxford New Hebrides Expedition returned to England in February of of 1934, Harrisson decided to stay behind. "To me the most important thing: that I found I could live native, or near it. Their food and sleep, kava, meal-times and laughter suited me. I never have been a shoe wearer, or natural shaver. This was useful. Because I soon spent the scanty money Oxford had given me to come home with. And then I was able to live on without; a year cost £4."

The author got his training (mainly in biology) at English universities, is 25 years of age, and has been explorer since the age of 18. Certainly this is in many ways a remarkable work, whether one chooses to consider it as a travel book or as a treatise on the anthropology of primitive peoples. In 1606 the Spaniards discovered the New Hebrides, and during the 200 years that followed French and English expeditions made sporadic contacts with various island groups in the archipelago. In 1828 came the discovery of sandalwood, and the consequent launching of a period of exploitation which was characterized throughout by as vicious treatment of native populations as one can find anywhere in history. Harrisson's account is detailed, sardonic, and strictly amoral. At frequent intervals remarks of a sort not customarily made by well brought up anthropologists are injected into the narrative.

"When he took over the parish he informed his parishioners, 'religion may be compared to a coconut. The husk of ignorance must be removed and the hard shell of the love of sin must be broken by the hammer of the Word, ere the blessing could be obtained. Once secured, it is indeed meat and drink to the perishing soul.' Years later it was to be meat and drink and millions with a peerage to Mr. William Lever."

"Geddie was a Nova-Scotian, real old style hardbit Presbyterian, a fine clean intolerant tough—one hundred per cent all talking, hymning and danceless."

The author managed to get along so handsomely with the natives that just at about the time of his departure plans were afoot to convert him into a chieftain. The descriptions of native life differ in two essentials from most studies of this sort; the author actually *went native* whereas other anthropologists, in the majority of cases, do not more than delude themselves into believing that they are living *like* natives, and secondly, the elaborate rationalizations so beloved by many anthropologists are here conspicuous by their absence.

The following is Harrison's candid opinion of the work of one of his predecessors:

"The standard work on the area is Rivers' *History of Melanesian Society*. This has been regarded as a great scientific work. That is a mistake. It is only great prose. It is a brilliant piece of circular subjective reasoning and creative literature. It is the result of a short study, mostly among mission natives on board a mission yacht. Volume one is filled with anthropological data. Volume two, the same size, contains the theoretical chat about the first volume. Everything in the New Hebrides is explained (though large areas are still culturally unknown) on a perfect pattern of logic (was Quiros logical?). Everything is brought by migrations of people, 'kava people' and 'betel people' especially (Rivers never studied the botanical distribution of the betel palm, and the fact that kava *Piper methysticum* is an exceedingly close relative of *Piper betle*, which may throw simple light on the reason why the 'betel people' did not get far south. . . .).

There is an index, and an excellent bibliography.

THE ABBÉ DU BOS—HIS ADVOCACY OF THE THEORY OF CLIMATE. *A Precursor of Johann Gottfried Herder.*

By Armin Hajman Koller. The Garrard Press, Champaign, Ill. \$1.75. $7\frac{1}{2} \times 5\frac{1}{4}$; 128; 1937.

The Abbé du Bos was a remarkable man—a diplomatic representative of France by turns in England, Spain, Italy, Germany, Holland, and Belgium; secretary of the French Academy; a rival claimant against Baumgarten for the honor of being the founder of the science of esthetics. Today he is practically unknown except by a few enthusiasts.

The present work is devoted to a discussion of a bizarre theory elaborated in his *Reflexions Critiques*, which was published in 1719 when he was 49 years old. It has very little to do with climate, but

concerns itself with the factors that produce artistic genius.

Du Bos confines his discussion to the representative arts. Vitruvius and Lulli are the only presentative artists mentioned at all, and of these the first is remembered today chiefly for his technical treatises than for his artistic creations, and the second is generally not remembered at all.

Very briefly, Du Bos believed that genius was produced by changes in climate. He refers to four periods in history when efflorescences of genius have occurred—the ages of Pericles, Augustus, Julius the Second, and Louis XIV. Each of these was strictly delimited in space as well as in time, and Du Bos believes that it can be shown that local changes in climate took place in each locality both before and afterwards. But the evidence he produces is very skimpy and not convincing. All racial characteristics he attributes to the climatic environment. The Portuguese and the Negroes in the Cape Verde Islands look alike, despite the fact that there has been no miscegenation (?). Also, when the Normans conquered England and when the Franks settled in France both peoples left behind their old climate and their old racial characteristics and adopted those o the new country. Homesickness is failure to become acclimated to a new environment.

His complete neglect of the Elizabethan age in English literature detracts from the value of his theory, and the unfortunate fact that he died too soon ever to have heard of Haydn, Mozart, Beethoven or Wagner weakens it. The author of the present work is a great admirer of Du Bos but this does not blind him to the latter's weaknesses, such as his discussion of the influence of industry on climate, the composition of the atmosphere, the effect of war on civilization, etc., all of which he brings in, and on all of which he goes astray, because he lived too soon.

The work has great value, however, as the discussion of eras of genius involves the discussion of the men who made it so, and this helps to keep alive many of the great names of the past which our modern method of life tends to consign to an undeserved oblivion.

Primitive Religion *Its Nature and Origin.*
By Paul Radin. The Viking Press, New York. $3.50. $8\frac{1}{2} \times 5\frac{3}{4}$; x + 322; 1937.

Dr. Radin's book is an excellent study on aboriginal religions for three reasons: he removes his sphere of interpretation from the "intellectual vacuum" of the psychologist to a study in "terms of human personalities", not pseudo-psychological generalities; he uses first-hand descriptions, quotations from native informants, and authentic descriptions given by scholars intimately connected with certain cultures; and he quotes at substantial length for the edification of the lay reader.

He first approaches his study of the nature and origin of primitive religion through an inimical social-economic background and states that religious feeling is an "emotional correlation of the struggle for existence in an insecure physical and social environment". Therefore, primitive religion was fostered in a maternal bed of social-economic origin. Consequently when life values are sustained by other means than religious phenomena religion is divorced from mundane affairs. Religious intensity is divided into three types: true, intermittent, and indifferent, with many gradations among the second group. His division of the "mana" concept into two groups, the personal or materialistic-magical side and the impersonal or idealistic-mystical aspect, is the essence of his theory of non-homogeneity in the religious experience among members of the primitive groups. A division, therefore, is made between shaman or priest and the layman. This antithetic feeling between religious experience and pure practical function runs as an underlying current through the religious life of simple cultures to complex agricultural civilizations, up through the concepts of the soul, ghosts, and gods, and through religious formulators, magicians, priests, their esoteric rituals, and ritual dramas. An excellent bibliography completes this scholarly study.

The Nature of Human Nature *and Other Essays in Social Psychology.*
By Ellsworth Faris. McGraw-Hill Book

Co., New York. $3.50. 9 x 6; xii + 370; 1937.

These rambling and loosely connected essays fall into five broad categories: 1. *Group and Person*, 2. *Conduct and Attitudes*, 3. *Sociology and Education*, 4. *Sociology and Ethnology*, and 5. *The Sociology of Racial Conflict*.

To a large extent the discussions center about words (never well-defined) that have been coined by various sociologists. Great effort is expended, with very little recourse to facts, in analyzing these concepts, and in determining their validity. The word *group*, as an example, is at the start assumed to have an independent existence, and then the search is on to find what it is that corresponds to the word. From the vaguely defined *group* we pass on to *primary* and *secondary groups*. *Sect*, *imitation*, *attitudes*, *punishment*, etc. receive the same treatment. The procedure is not to start with facts, and then try to search out the uniformities among them, but rather to make facts correspond in some way to words that were never precisely defined in the first place. Behaviorism, gestalt psychology, psychoanalysis, and the psychological experimentation of physiologists all serve to release series of observations that are sometimes more dogmatic than intelligent.

But Faris has his moments of shrewd and acute criticism.

> The central doctrine of the Unconscious (impressively capitalized) appears to be a hypostatization of the notion of the subliminal which is at least three hundred years old and has received recognition ever since. But the Unconscious is presented in the books of these men as the most important aspect of human life, a rather repulsive dungeon where evil spirits are confined, to be exorcised by letting the cat out of the bag. If proof of the existence of this limbo is demanded, reference is made to the maturation of problems, a phenomenon long familiar. Men have awakened from sleep to find a difficult solution all clearly apprehended, but it can also be said that a skater has suddenly found his performance improved, though this would not mean that the Unconscious had been exercising on the ice.

Pareto is disposed of as follows:

> Although the book has no value for sociology, the student of personality should find it a serviceable document. The unintentional revelation of Pareto's coarseness, his scorn for moral principles (2316[10]), his unfairness to opponents, his utter lack of a sense of humor, his towering egotism—all these and much more are obtrusively manifest. Some competent student should work through the material with a view to understanding the development of the personality of an old man who aspired to be the Machiavelli of the middle classes. One result of such investigation might be the explanation of why he thought he could teach the world sociology without ever having learned it, even if he must use a million words.

LATER CRIMINAL CAREERS.

By Sheldon and Eleanor Glueck. Commonwealth Fund, New York; Oxford University Press, London. $3.00. $9\frac{1}{4}$ x $6\frac{1}{4}$; xi + 403; 1937.

In 1930 the authors reported on the careers of 500 criminals after five years from their discharge from the Massachusetts Reformatory. After five more years this group has been surveyed again and the results of the investigation are presented in this volume. At the end of the second five-year period the number of apparently reformed criminals had increased from 89 at the end of the first five years to 118, that is, from 21.5 to 32.1 percent. The authors are particularly interested in trying to determine the factors associated with this increase. They have considered numerous familial and social characteristics including reformatory history, economic responsibility, intelligence, and others, but none of these seem to be correlated with the increase in reformation. *Faut de mieux* the authors conclude that since the criminals have grown older ageing is the factor responsible for reformation. Such a conclusion has very little, if any, meaning especially when from the data given it can be shown that the older criminals do not have a faster rate of reformation than the younger ones and that the age classes have contributed to the increase each in proportion to its number.

The book ends with a plea for a change in the peno-correctional system. With this everyone should agree. The study plainly indicates that after 10 years almost 70 percent of the original sample of ex-convicts are still criminally active, therefore there can be no doubt that something is radically wrong with the system. [Reginald the Office Boy says that this is a *non sequitur* if he ever saw one. The

lad becomes more useful to us every day.] While this book deals with a unique investigation on a subject of paramount interest it is a very difficult one to read. The presentation of the findings is particularly faulty.

Across Cyprus.

By Olive M. Chapman. With a Foreword by The Viscount Mersey. John Lane The Bodley Head, London. 15s. net. $8\frac{1}{2}$ x $5\frac{3}{8}$; 255 + 34 plates + 1 folding map; 1937.

A pleasant, well-written, quiet sort of travel book about a place that was much more exciting and important 2500 years ago than it is now. In a short opening chapter the author reviews the checkered history of Cyprus. One item in it has a certain pertinence at this moment. It shows once more how very recurrently human beings are wont to behave. Also it calls to mind those classic lines

How odd
Of God
To choose
The Jews.

In A.D. 116–117 there was an insurrection on the part of the Jews, who at that time comprised a considerable part of the population. They brutally massacred thousands of the Greeks, and it was two years before the revolt was finally suppressed by the Roman Governor Lucius. During that time a quarter of a million of the inhabitants of Cyprus are said to have been slain. All Jews were eventually expelled from the island by order of the Senate. So bitter was the feeling against them that for a great number of years afterwards no Jew was allowed to land in Cyprus, and if any of their race were found on the island they were immediately put to death, even though, as sometimes happened, they were shipwrecked near the coast and forced by circumstances to land. The result of this long-engendered spirit of revenge has left its mark, for as late as 1911 the total number of Jewish subjects in Cyprus amounted to only one hundred and ninety-three persons.

The Geography of Europe.

By George D. Hubbard. D. Appleton-Century Co., New York and London. $5.00. $8\frac{3}{4}$ x 6; xii + 876 + 1 folding map, 1937.

In these days of tension and general unrest, when "the political situation in Europe" is upon the tongue of everyone, it has been a relief and a pleasure to find a book which describes the countries of Europe calmly and dispassionately from the point of view "of the relation of man and his activities to the physical conditions in which he finds himself". Each of the twenty-seven major states of Europe is discussed separately from this angle, the physical conditions being classified by the author into three main categories: topography, soils, climate, distribution of land and water, elements of relief; resources, as forests, power, fuel, building materials, minerals, fisheries, wild life; neighbors and their social, economic and religious standards and status. The relation of man to these conditions is shown in a broad survey of his activities in response to such environmental features. Early geologic and physiographic history, and past races are also considered as conditioning influences on the activities of the present peoples.

While intended primarily as a text for geography students, this book with its wide range of information, its many maps, and numerous photographs well selected for depiction of scenes typical of the various countries, will be found by other specialists and the general reader alike to be both interesting and instructive. An index is included.

Naturalists of the Frontier.

By Samuel W. Geiser. With a Foreword by Herbert S. Jennings. University Press in Dallas, Southern Methodist University, Dallas. $3.00. 9 x 6; 341; 1937.

In this set of biographies Dr. Geiser has commemorated the careers of ten men who earned varying degrees of recognition for their work as pioneer naturalists and collectors in the southwest during the years 1820–1880. These ten contributors to the advancement of natural science in a new country were Jacob Boll, Jean Louis Berlandier, Thomas Drummond, Louis Cachand Ervendberg, Ferdinand Jakob Lindheimer, Ferdinand von Roemer, Charles Wright, Gideon Lincecum, Julien Reverchon, and Gustaf Wilhelm Belfrage. The author spared no effort in his search for accurate data on the lives and works of

these men and on the details of the history of the Texas frontier which serves as the background for these personalities. As a result of this painstaking preparation plus the author's gift for pleasant narration, we are presented with a book not only of permanent historical, biographical, and scientific value, but also of great interest.

In the first of two appendices, numerous bibliographical sources are listed for each of the ten biographies. In the second is a list containing the names of over 150 naturalists and collectors known to have worked in Texas during the period 1820–1888. Dates, notes and bibliographic references follow each name. There is also an index of names.

Human Affairs.

Planned and Edited by R. B. Cattell, J. Cohen and R. M. W. Travers. Macmillan and Co., New York and London. $4.25. $8\frac{1}{2} \times 5\frac{1}{2}$; xi + 360 + 17 portraits; 1937.

The editors, three young men with an evident flair for vivid advertising language, have revived the old idea that the Scientist or, as Plato put it, the Philosopher should direct human social behavior. Therefore, they tried to inveigle 14 eminent scientists representing a wide variety of fields to contribute each an article on the subject of what science can do to aid in the prevention and treatment of the ills of society. As usually happens, the scientists evaded the main issue and here each presents an article of a more or less general nature and not very inspirational regarding his own field of activities. A few have entered into the spirit of the occasion by tacking to the end of their articles a paragraph or two in which the scope of this symposium is restated. The 14 scientists represent biology (J. B. S. Haldane), psychology and psychiatry (D. Katz, E. Chambers, E. Miller and W. McDougall), social sciences (A. S. J. Baster, M. Ginsberg, K. Mannheim and the Earl of Listowel), anthropology (Lord Raglan and B. Malinowski), eugenics (C. P. Blacker), medicine (H. Brackenbury) and sexology (Havelock Ellis). The senior editor also contributes with an article on education. Each article is accompanied by a photograph of the author whose biography is briefly given in an appendix.

Research Memorandum on Crime in the Depression. *Bulletin 27.*

By Thorsten Sellin. Social Science Research Council, 230 Park Ave., New York. $1.00. 9 x 6; vii + 133; 1937 (paper).

Research Memorandum on Social Aspects of Health in the Depression. *Bulletin 36.*

By Selwyn D. Collins and Clark Tibbitts with the Assistance of Arch B. Clark and Eleanor L. Richie. Social Science Research Council, 230 Park Ave., New York. $1.00. 9 x 6; xiii + 192; 1937 (paper).

These are two of the series of monographs sponsored by the Social Science Research Council to stimulate the study of the social effects of the recent economic depression. The first reviews the results of some of the investigations on the relation between economic conditions and crime trends and critically discusses the sources of error which effect such investigations. The author outlines in some detail what he considers are the studies needed to illuminate the problem of the effects of economic changes both on the manifestations of criminality and on the making and execution of laws.

A similar pattern of exposition is followed by Collins *et al.* in their discussion of health and disease in relation to the depression. These authors summarize some of the observations made regarding changes in mortality and morbidity during the depression, and the relation of economic condition to health status. They also describe the extent of the organization of medical care by private and public means.

Socialized Medicine in the Soviet Union.

By Henry E. Sigerist. W. W. Norton and Co., New York. $3.50. $8\frac{1}{2} \times 5\frac{5}{8}$; 378 + 16 plates; 1937.

This is a portrayal of the progress of medicine and, in particular, of public health

measures in Soviet Russia. It is written with such exaggerated expressions of uncritical admiration as to appear in parts not the work of an eminent historian but rather the panegyric of a Rotarian speaking of "our fair city". In the first chapter is given a condensed discussion of Marxism and Leninism, followed by a description of the state of medicine and public health in czarist Russia. In the following chapters Sigerist reports on the present system of medical teaching and practice and the ways and means adopted to safeguard the public health. Without doubt there has been progress in the health conditions of Russia during the last 20 years, but how great it has been and to what extent it is an achievement possible only in a Soviet state is impossible to say. The Russian government is uncommonly reticent in publishing vital and health statistics and Sigerist gives none to speak of. Since the author's statements are based either on limited observations or else on plans which have yet to be executed, this book cannot be utilized as a means of realistically evaluating socialized medicine.

HANDBOOK OF NORTHERN ARIZONA POTTERY WARES. *Museum of Northern Arizona, Bulletin No. 11.*

By Harold S. Colton and Lyndon L. Hargrave. Northern Arizona Society of Science and Art, Flagstaff, Ariz. $4.00. $8\frac{1}{4}$ x $5\frac{1}{4}$; xiv + 267; 1937.

An attempt has been made in this work to analyze the pottery types that are found on the pleateau of Northern Arizona. Large numbers of sherds have been found in this area, but the lack of a system of classification has resulted in a great confusion of descriptions and names. In an effort to bring order out of chaos, and to establish a standard technique of procedure for workers in the field, the authors have defined a few terms (such as "type", "ware", "series") which have hitherto been rather loosely used; have proposed rules for the determination of names to be adopted; have sorted and analyzed the available mass of material, and have constructed a key for use in the identification of southwestern pottery types. The major portion of the book is devoted to descriptions of pottery types and wares of the area delimited, each description following a standard arrangement for the presentation of each character. Glossary, bibliography, and index are provided.

EARLY MAN *As Depicted by Leading Authorities at the International Symposium at the Academy of Natural Sciences, Philadelphia, March 1937.*

Edited by George Grant MacCurdy. Introduction by John C. Merriam. J. B. Lippincott Co., Philadelphia. $5.00. 9 x 6; 362 + 27 plates; 1937.

This volume presents the contributions of foreign and American investigators to a symposium on Early Man. The fact that 36 papers were read is indicative of the success of the congress, but the list of authors is not entirely representative of the world's students of the problem. The subject matter includes papers on the relics of Early Man in Java, America, the Near East, Norway, China, India and Australia. There are also a number of articles discussing problems of Pleistocene stratigraphy. The eminence of the authors, among whom are included Hrdlička, Keith, Gregory, and Dubois, is a sufficient guarantee of the quality of the papers which, in general, are written in an interesting manner. The majority of papers dealing with Early Man proper report on the evidence of the antiquity of man in America. In view of some differences of opinion among the students, a final summary of all the facts and theories on this topic would have been helpful.

THE ANATOMY OF MURDER. *Famous Crimes Critically Considered by Members of the Detection Club.*

By Helen Simpson, Margaret Cole, Dorothy L. Sayers, John Rhode, E. R. Punshon, Francis Iles and Freeman Wills Crofts. The Macmillan Co., New York. $2.50. $8\frac{5}{8}$ x $5\frac{5}{8}$; vii + 336; 1937.

This volume consists of the stories of seven murders—some famous, others un-

known to the general public—written by an equal number of members of the Detection Club. As is only natural in such a collected volume, there is a great diversity both in the style of the writers and in the manner in which the crimes are treated. Some of these studies offer an excellent opportunity for the reader of detective stories to exert his mind in piecing together fragmentary evidence, while others offer fascinating character studies for those more interested in the personality and motives of the criminal than in the mere mechanics of the crime.

The work of Dorothy Sayers and that of Francis Iles is especially commendable. The former writes of an almost inconceivable murder in a humdrum and respectable home, and the latter of an exotic crime in a weird and completely abnormal household. Most of these accounts rise far above the average murder mystery, and several are not only excellently, but also amusingly, written.

Life as a Whole.

By J. W. Bews. Longmans, Green and Co., New York. $6.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; ix + 347; 1937.

In this book the author, professor of ecology in Natal University College, approaches the study of the life of man with ecological methodology—separate excursions guided by expert leaders into various aspects of life, collection of facts and observations, and finally a synthesis of the parts into an integrated whole. The scope of the book is remarkable. Viewpoints glanced at include physical chemistry, evolution, neurological physiology and psychology, and endocrinology. Autecology (the life history of man, with emphasis on transitional periods), from birth to old age is studied. Man's works, a reflection of his life in the world, are considered—his material works, his social life and institutions, and his philosophies. Finally the study of man's art, architecture, sculpture, painting, music and great literature is undertaken. The material is necessarily presented briefly, but with commendable objectivity and balance. An excellent bibliography is appended and the book is well indexed.

Pression Solaire et Astrophysique. Les Maladies de la Nutrition. *L'Eau en Physique et Biologie.*

By G. Froin. Girardot et Cie, Paris. 35 francs. $9\frac{1}{2}$ x $6\frac{1}{4}$; 352; 1937 (paper).

This is the last of a series on astrophysical influences on life, dealing in particular with solar, lunar and other astral factors in the causation of nutritional diseases. These are too complicated to treat adequately in this review. However, incidentally, the author has found an ingenious explanation of the color of the various races by the hypermagnetic light waves from the planets diffracted through the moon to different areas of the earth. Thus, red and orange waves striking the Americas explain the "red" of the Indians. [Footnote 1: "It is probable that the whites who have migrated to America will in time assume a red color, provided they do not cross-breed with other whites and yellow races."] Yellow rays landing in Eastern Asia account for the skin-color of the Mongolians; the greens and blues strike Europe, forming the complimentary white. Africa receives no magnetic light waves, and hence its inhabitants are black.

Rapport sur le Pèlerinage du Hedjaz *de l'Année de l'Hégire 1355 (A.D. 1937).*

Conseil Sanitaire Maritime et Quarantenaire d'Egypte, Alexandrie. Free. $12\frac{1}{2}$ x $9\frac{1}{4}$; 147 + 5 plates + 5 folding tables; 1937 (paper).

The flight of Mohammed from Mecca to Medina in 622 A.D. is celebrated annually by a pilgrimage of faithful Mohammedans to Hedjaz. During the past decade, a careful study has been made of the general health of the pilgrims, and of the hygienic and sanitary conditions under which they make the journey. This paper is the tenth annual report, the plan of which was originated at the International Sanitation Convention of Paris in 1926.

The study includes a statistical report of the numbers of pilgrims from the various countries, by sex; the methods of travel; a description of the sanitary conditions of boats; a discussion of the medical precautions taken; and the morbidity and mortality during the pilgrimage. In

comparison with reports of previous years, the report for 1937 shows that ever increasing precautions are taken to safeguard the health of the travellers, and that the mortality from infectious diseases among them is almost nil.

Adventures in the East.

By Lili Körber. John Lane The Bodley Head, London. 12s. 6d. net. 8½ x 5½; 347; 1937.

The original German volume *Begegnungen in Fernem Osten* from which this translation was made, was first published in 1936, following the author's return from a few months' visit in China and Japan. The trip had been an objective one, made for the purpose of a close range study of political and social conditions in these countries, and this book records the results of such a study as made by a wide-awake, intelligent and highly observant newspaper woman. But while the serious effort to portray an illuminating picture of the forces at work in shaping the history of these nations is underlying throughout the text, the author has by no means limited herself to a discussion of grave problems. Vitally interested in all matters of the moment, and intrigued by people as individuals, she tells a delightful story of her experiences as a traveler and adds much of human interest in the story of her social contacts and friendships made during her visit to the Orient.

Mortality Trends in the State of Minnesota.

By Calvin F. Schmid. University of Minnesota Press, Minneapolis. $3.50. 9 x 6; ix + 325; 1937.

This is a thorough and fairly complete exposition of the changes in mortality as observed in Minnesota from 1910 to 1935. From the official vital statistics, mainly, the author has compiled data regarding the trends of the total mortality in Minnesota according to cause, age, sex and season and when possible compared these trends with those for the United States. Data on infantile and maternal mortality are also presented and compared with those for the population of the United States as a whole. A more detailed study of the mortality in Minneapolis and St. Paul is also included. There is an appendix containing information regarding metereologic conditions of the state and the description of a method of allocating births and infant deaths according to residence. The basic data here presented should be very useful as a starting point for investigations on the demography of Minnesota.

Colonial Population.

By Robert R. Kuczynski. Oxford University Press, London. $1.75. 8¾ x 5½; xiv + 101; 1937.

This is a compilation and discussion of official data on the total population of all colonies and mandated areas, as well as its distribution by race and by continent of birth. A brief description of the state of birth and death registration in each colony is also included. As the author points out in his introduction the official data for different colonies are of very unequal value. A few are based on carefully taken censuses, others on censuses inadequate in execution or including only part of the population, others on estimates of varying adequacy—as late as the 1920's the estimate of the population of Hong Kong was based on the amount of night-soil collected—while still others lack any factual basis. The student of population may well be grateful to the author for assembling in one volume data for which he would otherwise have to hunt through many publications, some of them not easily obtainable.

The Negro's Struggle for Survival. *A Study in Human Ecology.*

By S. J. Holmes. University of California Press, Berkeley. $3.00. 9¼ x 6; xii + 296; 1937.

Holmes examines the trends of birth, death and growth rates of the Negro population of the United States. He believes that the increased growth rate be-

tween 1920 and 1930 was real and was stimulated by the decrease in foreign immigration. If the restrictions against foreign immigrants are maintained and the birth-rate of the whites of the rural south continues to decrease it is probable, the author believes, that the Negro population will increase at an even greater rate. In addition, if the present Negro morbidity and mortality can be considerably reduced then the United States will be faced with the problem of how to regulate the Negro-white population balance. The author is well aware that at present there is no adequate basis for future predictions and therefore he limits himself only to an outline of the possibilities. The book contains an extensive bibliography.

CAPTAINS AND MARINERS OF EARLY MARYLAND.

By Raphael Semmes. Johns Hopkins Press, Baltimore. $5.00. $9\frac{1}{2} \times 6\frac{3}{4}$; xvi + 856; 1937.

An account of various phases of the social history of seventeenth-century Maryland, largely verbatim quotations from the original sources. We are still uncertain whether the "Captains" of the title refers to Captains of vessels or of militia companies. Both subjects are dealt with, as well as the game that furnished the colonists with much of their meat, the fur trade, the frequent disputes between the Governors and the Assembly, the struggle between Claiborne and the authorities of Maryland over the possession of Kent Island, and the relations of the colonists to the neighboring Indian tribes. There is little attempt at synthesis, but the reader receives a vivid impression of life in early Maryland.

THE IRISH COUNTRYMAN. *An Anthropological Study.*

By Conrad M. Arensberg. Macmillan Co., New York. $3.00. $8\frac{1}{2} \times 5\frac{3}{8}$; xi + 216; 1937.

This book is a series of Lowell Institute lectures. There are chapters on the countryman at his work, the family and its relationship to land holdings, the ties between family members, and the functions of shops, pubs, and fairs in Irish rural community life. The material is well organized, and the style of writing, even when the author holds forth upon the new anthropology, is unusually clear. In the author's view anthropology "has become a behavioral, or better, an operational science, to use a term Bridgman has introduced into the natural sciences." It is not, however, easily apparent from the results that the concrete application of this philosophy has led to any strikingly different conclusions than have been reached by older anthropological and sociological approaches.

There is no index.

THE HUMAN BODY. *Third Edition, Corrected, Enlarged, and Rewritten.*

By Logan Clendening. Illustrations by W. C. Shepard and Dale Beronius. Alfred A. Knopf, New York. $3.75. $9\frac{1}{4} \times 6\frac{1}{4}$; xv + 443 + ix; 1937.

This book appeared before us ten years ago, and after having been corrected and enlarged has returned to us again for our edification. In his foreword Dr. Clendening announces that time and increased knowledge have caused him to change practically all of his opinions and he has rewritten this book accordingly. It has enjoyed wide and deserved popularity, and bids fair to go merrily along its way for a considerable spell longer. All in all we know of no book trying to do the same thing for the general reader that is so entertaining and dependable.

A GRAPHIC SUMMARY OF THE VALUE OF FARM PROPERTY. *(Based Largely on the Census of 1930 and 1935.) U. S. Department of Agriculture. Miscellaneous Publication No. 263.*

By B. R. Stauber and M. M. Regan. Government Printing Office, Washington. 5 cents. $9\frac{1}{8} \times 5\frac{7}{8}$; ii + 20; 1937 (paper).

This is a series of 24 maps and graphs showing the drastic changes in farm property values that have occurred during the

last quarter of a century. Figures 1–4 deal with the geographic distribution of the total value of all farm property and its division according to classes. Figures 5–15 show various aspects of the changes in value of farm real estate, while figures 16 to 21 illustrate the distribution as well as changes in the value of farm real estate —lands and buildings. The three final figures deal with farm-dwelling values for 1930 only, the one year for which data were available.

TASCHENBUCH DER RASSENKUNDLICHEN MESSTECHNIK. *Anthropologische Messgeräte und Messungen am Lebenden.*

By Bruno K. Schultz. J. F. Lehmanns, Munich and Berlin. 6 marks (In Germany); 4.50 marks (Outside of Germany). 7½ x 5; 102; 1937.

This is a pocket sized hand-book based on Martin's two volume text of anthropology. The author has divided the book into two sections, one dealing with the instruments and technique of taking measurements and the other with the technique of body measurements on the human body. At the end of each section references are given to Martin and other authors. The book should be of great use in the class-room because of its convenient size and the ease with which it can be consulted.

LE PROFIL GRAPHIQUE DES INDIVIDUS ET DES GROUPES. *Normalité et Anormalité. Actualités Scientifiques et Industrielles 423. Exposés de Biométrie et de Statistique Biologique, X.*

By Alfredo Niceforo. Hermann et Cie, Paris. 12 francs. 10 x 6½; 51; 1937 (paper).

The author discusses a graphic method of representing synthetically how an individual or a subgroup deviates with respect to a series of characteristics, body measurements for example, from the general population. The method has already been adopted by psychologists and the author illustrates its application to various studies in that field as well as to studies in anthropology and demography. It is a useful statistical tool.

AFRICA'S GOD. *VI—Uganda. Anthropological Series of the Boston College Graduate School, Vol. II, No. 3.*

By Joseph J. Williams, S.J. Boston College Press, Chestnut Hill, Mass. $1.00. 9¼ x 6½; 44; 1937 (paper).

The author comes to the conclusion: "that in Uganda the Supreme Being of former days has lost much of His standing by the condition of retrogression in religious practices;" and, "there is much to be said in favor of the supposition of Monsignor Gorju that the monotheistic beliefs of Uganda are to be attributed to a Hamitic influence which was brought in from Christianized Nubia."

THE HIGHVELD CLIMATE.

By L. E. Hertslet. Publicity and Travel Bureau, South Africa House, London. 9⅝ x 7⅞; 8; 1937 (paper).

"A Challenge to the World!" This pamphlet consists of six pages of statistics on the sunshine, temperature, wind, and other atmospheric conditions of the Highveld of South Africa, and boasts of its modern conveniences, its accessibility to other parts of the world, and its scenic and scientific interests.

DIE EIGENWELT DES MENSCHEN. *Bios, Band VIII.*

By Hans Petersen. Johann Ambrosius Barth, Leipzig. RM. 2.25. 9¼ x 6¼; 28; 1937 (paper).

This is a philosophical treatment of biocenosis, attempting to apply von Uexküll's environmental theory to man. The author attributes to Plato instead of Aristotle the expression that man is a *Zoon politicon.*

ZOOLOGY

WILD ANIMAL WORLD. *Behind the Scenes at the Zoo.*

By Raymond L. Ditmars and William Bridges. D. Appleton-Century Co., New York and London. $3.00. 8¼ x 5½; x + 302 + 16 plates; 1937.

MAMMALS OF CIRCUS AND ZOO. *Including a Curriculum Unit on Mammals of Circus and Zoo Developed in the Saratoga Union School. Science Guide for Elementary Schools, Volume III, Number 6.*

By Edith A. Pickard. California State Department of Education, Sacramento. 15 cents. 9 x 6; iii + 57; 1937 (paper).

In the first of these books the authors have drawn aside the hypothetical curtain that conceals from public gaze the most interesting part of the modern zoo. They personally conduct us through the hospital, into the isolation room where the incoming acquisitions are quarantined, to the diet kitchen where meals are prepared for each according to his kind, the accident ward where the chimpanzee had a plaster cast put on its broken arm, to the surgical ward where the tiger had its teeth filled and the rhinoceros had its cataracts removed. For the modern zoo is not merely a living museum—it is a scientific institution where the inmates are not only exhibited but given the care they need to keep them fit, and where their needs and habits may be learned from observation.

There is much more to running a zoo than appears on the surface. New acquisitions may be made from time to time by purchase or exchange with other zoos, but the business of importing living animals from the jungle or the tropical islands is now so highly developed that the best exhibits are always brought in fresh from the wilds. These authors take us by proxy to the ends of the world and back, and in imagination we assist in the capture of the Komodo lizard, the Galapagos tortoises, the sea snakes (there really are sea serpents from the tropical Pacific), the Australian earthworm that is twelve feet long that Dr. Ditmars has never seen but hopes to exhibit some day, and finally we make the acquaintance of the strangest of all the animals in the zoo—the human animals that ask foolish questions and tell the director he is "all wet" because he doesn't believe that snakes swallow their young for protection and disgorge them later when danger is past, or who wish to obtain animals for vaudeville skits, or to make presents of pets that can no longer be kept at home. This book will be found interesting reading by everybody, but especially by those who like the present reviewer have had the privilege of working in a zoo themselves.

The second work is a manual for teachers who wish to give children an appreciative knowledge of what they may see in a zoo. It is a number of a monthly publication prepared by a group of teachers to help other teachers. It is highly condensed, consists largely of descriptive matter but also contains instructions on how to teach elementary zoology. Outside the teaching profession it is likely to have but little appeal.

MOSAICS AND OTHER ANOMALIES AMONG ANTS.

By William Morton Wheeler. Harvard University Press, Cambridge. $2.00. 9¼ x 6; [12] + 95; 1937.

The manuscript of this book by our lamented co-editor was delivered to the publisher only a few days before his death. It is a contribution of the first rank of importance, fittingly capping the life work of one who was universally recognized at his death as the foremost student of ants in the world.

The volume deals with a colony of a fungus-growing Attine ant, *Acromyrmex octospinosus* Reich, collected by Dr. Neal Weber in Trinidad. Out of a total population of 8174, it contained 164 anomalous individuals, 53 of which are unlike any previously observed anomalies among ants or among any other social insect. These exceptional anomalies appear, under Wheeler's masterly analysis, to be decisive relative to the ancient and much debated problem of caste determination. Part I describes the distribution, habits, and normal castes of *Acromyrmex;* Part II, the anomalies of *Acromyrmex octospinosus.* Part III, discusses the long controversy over the trophogenic and blastogenic theories of caste determination. The general conclusion is that: "The caste, which could have arisen in the remote

past only in response to a very special social environment and can still maintain itself only in such an environment, has somehow become genotypic."

Appendices include taxonomic notes on *A. octospinosus*, and a revision of the known non-mosaic female and worker ant anomalies.

ARISTOTLE: PARTS OF ANIMALS, MOVEMENT OF ANIMALS, PROGRESSION OF ANIMALS.

Translated by A. L. Peck and E. S. Forster. Foreword by F. H. A. Marshall. Harvard University Press, Cambridge; William Heinemann, London. \$2.50 (cloth); \$3.50 (leather). $6\frac{1}{2}$ x $4\frac{1}{2}$; viii + 556; 1937.

The English speaking world is greatly indebted to Dr. Peck and Mr. Forster for the translation of these fascinating treatises by Aristotle. The volume presents (1) an introduction explaining the sources of the work, and giving reasons for various meanings given words where literal translation would have been misleading, and (2) the original Greek manuscript which, page for page, is followed by the English translation.

In the *Parts of Animals*, Aristotle concerns himself with the causes that have been responsible for animals being built the way they are, and with the functions of all the parts of animals. Of course many of his conclusions are erroneous in the light of our present biological knowledge, but what is more important is that Aristotle had very keen powers of observation, and used them adequately in studying an enormous number of organisms.

In the *Movement of Animals* and *Progression of Animals* the problems of the origin of movement, and the use of certain organs in the production of motion form the bases of discussion. In each of these dissertations, as in the former one, Aristotle shows amazing ability to observe and discuss a great number of characteristics of man, beast and birds.

A PICTORIAL GUIDE TO THE FAMILIES OF BIRDS (*Including a List of the Birds of Southeastern Michigan with Their Migration Dates*). *Cranbrook Institute of Science, Bulletin No. 9.*

By Edward T. Boardman and Elizabeth Barto. Illustrated by Vera K. Boardman. Cranbrook Institute of Science, Bloomfield Hills, Mich. 50 cents. 9 x 6; 48; 1937 (paper).

This outline was prepared for use in studying the birds of Michigan, but it contains much that is of value to the beginner in identifying and classifying the birds of any section of the country. The guide is to be used in acquainting the beginner with the various groups of birds, after which it is to be discarded in favor of the more classical handbooks.

The work presents in a very brief and elementary form the distinguishing characters of 19 orders including 48 families of birds native to Michigan together with sketches of head and feet and, in most cases, with sketches of the whole bird as it appears in some characteristic position. Also included are a check list of birds of Southern Michigan; a table showing the mean date and earliest appearance of the migratory birds; and a list of field guides and general reference books of birds east of the Mississippi.

LES HUILES DE FOIE DE MORUE. *Leur teneur en vitamines A et D.*

By Paul Chabre. Preface by A. Chevallier. Masson et Cie, Paris. 36 francs. $9\frac{3}{4}$ x $6\frac{1}{2}$; 207; 1936 (paper).

This is a very interesting treatise dealing solely with cod liver oil. All phases of the subject are included—the life history and appearance of the fish responsible for the production of the oil, the fishing areas, the manner of catching the fish, the extraction and purification of the product, its chemical and physical properties, and its economic importance. Special emphasis is given to vitamins A and D. The content of these important substances in the various kinds of fish is shown to vary considerably. Liver oil taken from fish that are caught off Newfoundland have more vitamin A content than those caught off the Norwegian coast. This is said to be due to the physiological state of the fish. Newfoundland fishing

occurs when the fish are well nourished and the liver is full of oil, while the Norwegian fish are caught when they are undernourished and thin. Excerpts from the pharmacopeia of various countries pertaining to cod liver oil, and a bibliography are included.

MY FRIEND THE ROOK.

By T. S. Hawkins. James Clarke and Co., London. 6s. net. $7\frac{3}{4}$ x 5; 201 + 8 plates; 1937.

The rook has many friends as well as enemies among Englishmen. In this volume are arrayed the "pros" and "cons" for this clever bird. One gathers that the chief objection comes from the farmer who fears that the rook is devouring his newly sown grain when it is really filling its crop with wireworms (larvae of the click beetle), the grub of the crane fly, etc., which find their way into the drill-hole. The author admits that the rook may do some harm to crops but he believes (as do many others) that it does far greater good in destroying the enemies of field plants.

Included in the volume are many interesting accounts of the behavior of these remarkable and likable birds and their seeming attachment to human beings and human habitations. One of the aims of the author is to dispel the curious lore that has long led to the slaughter of rooks in wholesale numbers at certain times of the year. The volume contains some interesting illustrations but is without an index.

AN ABRIDGED CHECK LIST AND BIBLIOGRAPHY OF WEST NORTH AMERICAN MARINE MOLLUSCA.

By A. Myra Keen. Stanford University Press, Stanford University, Cal. $1.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; 87; 1937 (paper).

This book is valuable to biostratigraphers as a source of latitude with midpoints-of-range corrected to date and to conchologists both as a working reference or check-list and as a compilation of literature printed from 1908 to 1936 on West American recent Mollusca. The excellent bibliography is divided into six compendia: recent literature alphabetically by authors and systematically by titles; recent revisions and synopses; indispensable publications; papers on statistical methods; and map lists of the East Pacific Coast. In addition, the book contains a check-list of alphabetically arranged genera with their respective species. The author lists sub-genera without species, but as cross references to genera in order to facilitate rapid unconfused reference. Univalves and bivalves are considered separately in the check-list; chitons and cephalopods are altogether omitted. A brief discussion of statistical methods in conchology is also included.

BOMBYLIIDAE OF PALESTINE.

By E. E. Austen. British Museum (Natural History), London. 15s. $10\frac{1}{2}$ x $7\frac{1}{4}$; ix + 188; 1937.

Of the 128 species or varieties of Bombyliidae dealt with, forty-six in addition to one genus out of 31 genera, are described as new. In the appendix particulars are given of ten species or forms of these flies which probably also occur in Palestine. Many species are protectively colored (greyish, tawny-olive or sandy) when seen against the soil as a background. But other species are deep black, and whether at rest or in motion are always conspicuous. The economic importance of the Bombyliidae would seem to be very great. All species of which the life history is known, are parasites of other insects and in many cases the host is a migratory locust. Up to the present the attempt has not been made to utilize any of the Bombyliidae in combating locusts since no method has been developed which is successful in breeding the flies artificially.

DIE BLATT-MINEN MITTEL- UND NORD-EUROPAS EINSCHLIESSLICH ENGLANDS. *Bestimmungs-Tabellen aller von Insekten-Larven der verschiedenen Ordnungen erzeugten Minen. Lieferung 4 and 5.*

By Martin Hering. Gustav Feller, Neubrandenburg. Subscription price for 6

numbers: (Germany and Switzerland) 12 marks; (foreign, except Switzerland) 9 marks. 9½ x 6¼; Lieferung 4, 337–448 + 2 plates; Lieferung 5, 449–560; 1937 (paper).

These two issues are continuations of Hering's taxonomic study of the larvae of the leaf-mining orders, which include Lepidoptera, Hymenoptera, Coleoptera, and Diptera. Part IV includes plant hosts indexed alphabetically from *Myrica* to *Rubus;* Part V, from *Rubus* to *Zinnia.* The illustrations show the shapes of the mines produced and the distribution of fecal material in relation to the pathways of the larvae. This work is a much needed contribution to the taxonomy and biology of leaf miners and is a valuable aid for preventive work in agriculture and forestry.

CROW-WATERFOWL RELATIONSHIPS. *Based on Preliminary Studies on Canadian Breeding Grounds. U. S. Department of Agriculture. Circular No. 433.*

By E. R. Kalmbach. Government Printing Office, Washington. 10 cents. 9⅛ x 5⅞; 35; 1937 (paper).

During the crow and duck-breeding seasons of 1934 and 1935, 512 duck nests located in three selected areas of southern Alberta and Saskatchewan were observed for evidences of destruction by crows. Field observations were supplemented by stomach examination. A high percentage of egg destruction was noted, and stomach examination revealed four times as many birds and eggs eaten by adult crows of these regions as by crows living under average conditions in this country. Various factors influencing this destruction are considered and recommendations are suggested for future crow-control operations on duck-breeding grounds.

NATURGESCHICHTE DER NORDATLANTISCHEN WALE UND ROBBEN. *Handbuch der Seefischerei, Bd. III, Heft 1.*

By Ernst Hentschel. E. Schweizerbart'sche, Stuttgart. RM. 15 (in Germany); RM. 11.25 (Outside of Germany). 10¼ x 7½; [6] + 54 + 10 plates; 1937 (paper).

Concise and practical, this handbook presents the essential features of a subject which is limited by lack of material for comprehensive study. Dr. Hentschel discusses the general body structure and habits of the whalebone and toothed whales, and of the walruses and seals, with specific data concerning individual forms within these groups. Little is said of their economic importance. The work contains numerous photographs and sketches. There is also an index and list of references.

HYDROIDS OF THE PACIFIC COAST OF CANADA AND THE UNITED STATES.

By C. McLean Fraser. University of Toronto Press, Toronto. $2.50. 9⅛ x 6¼; 207 + 44 plates; 1937.

This volume contains descriptions of every hydroid species (236 species, 59 genera) known to occur along the Pacific coast of Canada and the United States together with its distribution within the area. Keys to the families, species, and genera are given. A new genus and species, four new gonangia, and two species new to the coast are described and recorded. There is an index and forty-four plates of line drawings that illustrate all of the species listed.

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society, Volume XXII, Part 3, Numbers 14–20.*

New York Zoological Society, Zoological Park, New York. $1.40. 10½ x 7⅛; 99 + 6 plates; 1937 (paper).

The seven papers presented in this issue of *Zoologica* deal with a preliminary list of Bermuda deep-sea fish; echinoderms and hermit crabs collected by members of the Templeton Crocker expedition in the Gulf of California and on the West Coast of California; the caudal skeletons of some Bermuda shallow water fish; notes on feeding methods of the vampire bat; growth of Galapagos tortoises *Testudo vicina* (from 1928–1937); and lymphocystic disease in the adult common angelfish.

RECENT ADVANCES IN ENTOMOLOGY. *Second Edition.*

By A. D. Imms. P. Blakiston's Son and Co., Philadelphia. \$5.00. $7\frac{7}{8} \times 5\frac{1}{4}$; x + 431; 1937.

The second edition of this excellent book has been enlarged to the extent of 56 pages. Twenty-seven new illustrations are included and 18 of those in the earlier edition have been discarded. The sections which have not needed revision have been left practically unchanged, but other sections have received much revision. Such changes appear in the discussion of head segmentation, the genitalia, homologies of certain of the appendages and their parts, hormones and metamorphosis, palaeontology, visual stimuli, stimulatory organs, biological races, biological control, etc.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 466. Abt. IX, Methoden zur Erforschung der Leistungen des tierischen Organismus, Teil 3, Heft 7 (Schluss). Methoden der Vererbungsforschung.* Containing following articles: *Methoden zur Züchtung von Drosophila,* by G. A. Lebedeff; *Methoden der Erforschung der Vererbungsvorgänge bei Pflanzen,* by F. G. Brieger; *Methoden und Ergebnisse bei der Züchtung von Tetriginae,* by Robert K. Nabours.

Urban and Schwarzenberg, Berlin. RM. 17. 10 x 7; 288; 1937 (paper).

This volume contains articles on methods of breeding *Drosophila* and *Tetriginae,* and methods of investigating inheritance in plants. In the latter article the author attempts to separate the fundamental features of plant genetics. He discusses cytology and also the statistical treatment of genetic material.

FUR-BEARING MAMMALS OF CALIFORNIA. *Their Natural History, Systematic Status, and Relations to Man. Contribution from the Museum of Vertebrate Zoology, University of California. In two volumes.*

By Joseph Grinnell, Joseph S. Dixon and Jean M. Linsdale. University of California Press, Berkeley. \$15.00 for two volumes. $10\frac{1}{4} \times 6\frac{3}{4}$; xii + xiv + 777 + 13 plates; 1937.

These volumes were apparently written primarily for Californians with a direct financial interest in fur, but are entertaining reading for anyone. While there is every evidence of the material being carefully and accurately collected, it is presented in a light vein and is full of anecdotes by trappers of the region. It is the sort of book that one can read for an idle half-hour. The illustrations are excellent.

THE BIRDS AND MAMMALS OF THE WESTERN SLOPE OF THE AZUERO PENINSULA (REPUBLIC OF PANAMA). *Scientific Publications of the Cleveland Museum of Natural History, Volume VII.*

By John W. Aldrich and Benjamin P. Bole, Jr. Cleveland Museum of Natural History, Cleveland. \$1.75. $9\frac{1}{4} \times 6\frac{1}{8}$; 196; 1937 (paper).

The ecology of the Azuero peninsula is briefly discussed and it is indicated that its fauna is slightly more related to the western end of Panama (Chiriqui) than to the eastern. Annotated lists of the birds and mammals collected and observed, with descriptions of new forms and critical notes on others, are given.

ANIMAL TREASURE.

By Ivan T. Sanderson. The Viking Press, New York. \$3.00. $9\frac{1}{4} \times 6\frac{1}{4}$; 325; 1937.

Biologists will find a devastatingly adequate appraisal of this book by Mr. Loveridge, published under the title "If the Blind Lead the Blind Shall . . . ? or Reflections on Recent Reviews of 'Animal Treasure' " in the January number of our esteemed contemporary *The Scientific Monthly* (Vol. 46, pp. 16–24, 1938).

STUDIES ON THE BIOLOGY OF THE CRAYFISH CAMBARUS PROPINQUUS GIRARD. *Illinois Biological Monographs, Vol. XV, No. 3.*

By William C. Van Deventer. University of Illinois Press, Urbana, Ill. \$1.00. $10\frac{1}{2} \times 7$; 67; 1937 (paper).

This is a very complete account of the habits and life cycle of the crayfish. The studies were made primarily from field

observations rather than in the laboratory. Growth, reproduction, and seasonal population trends are given particular attention.

THE SPRUCE GALL APHID (ADELGES ABIETIS LINNAEUS) IN SOUTHERN MICHIGAN. *University of Michigan, School of Forestry and Conservation. Circular No. 2.*
By Bill Howard Wilford. University of Michigan Press, Ann Arbor. 20 cents. 9 x 6; 34; 1937 (paper).

THE WALKING STICK AS A FOREST DEFOLIATOR. *University of Michigan, School of Forestry and Conservation. Circular No. 3.*
By Samuel A. Graham. University of Michigan Press, Ann Arbor. 20 cents. 9 x 6; 28; 1937 (paper).

THE MORPHOLOGY, DIVISION, AND CONJUGATION OF THE SALT-MARSH CILIATE FABREA SALINA HENNEGUY. *University of California Publications in Zoology, Vol. 41, No. 25.*
By John Marshall Ellis. University of California Press, Berkeley. 75 cents. 10¼ x 6¾; 34 + 5 plates; 1937 (paper).

CYTOLOGICAL VARIATIONS IN THE BLOOD AND BLOOD-FORMING ORGANS OF WHITE-FOOTED MICE EXPERIMENTALLY INFECTED WITH TRYPANOSOMA CRUZI. *University of California Publications in Zoology, Vol. 41, No. 26.*
By Sherwin F. Wood. University of California Press, Berkeley. 50 cents. 10¼ x 6¾; 30 + 3 plates; 1937 (paper).

MORPHOLOGY OF THE POCKET GOPHER: MAMMALIAN GENUS THOMOMYS. *University of California Publications in Zoology, Vol. 42, No. 2.*
By John Eric Hill. University of California Press, Berkeley. $1.00. 10¼ x 6¾; 91; 1937 (paper).

BOTANY

TEXTBOOK OF DENDROLOGY. *Covering the Important Forest Trees of the United States and Canada.*
By William M. Harlow and Ellwood S. Harrar. McGraw-Hill Book Co., New York. $4.50. 9 x 6; xiii + 527; 1937.

This is a taxonomic treatise intended for the commercial forester, and consequently the vernacular names used are those derived from the trade rather than from popular usage, except in a few instances where the trade name seems unusually objectionable. The use of such terms as oak, larch, poplar, cedar, and cypress each for two distinct groups of trees is of course to be deprecated, but frequently the authors had no other choice In such cases they give notes of warning, but it cannot help but be confusing to the reader to find the names "laurel" and "arbutus" applied not to the plants commonly designated by those names, but to the aguacate and the madrone respectively. Incidentally, how did the name "arbutus" come to be applied to the mayflower in the first place?

Only native trees are covered, and of these only those which serve utilitarian purposes. There is no mention of the numerous naturalized acacias and eucalypti, and none of the palms. The gingko appears, on account of its singularly isolated taxonomic position. Also, there seems to be some doubt as to what constitutes a tree. The poison ivy is conspicuous, but the closely related round leaved sumach which grows like a bush is omitted.

The trees which are included are well described, and their identification will offer no difficulty. Their leaves, bark, fruit, flowers, and seeds are well illustrated with photographs. The authors seem to have leaned heavily on the classical work of Sargent, even to reproducing one of the typographical errors of that work. A bibliography of eleven pages and an index of nineteen pages completes the work.

USEFUL PLANTS AND DRUGS OF IRAN AND IRAQ. *Field Museum of Natural History, Publication 387. Volume IX, Number 3.*
By David Hooper with notes by Henry Field. Field Museum of Natural History, Chicago. $1.50. 9½ x 6¼; 173; 1937 (paper).

THE NORTH AMERICAN SPECIES OF RUMEX. *Field Museum of Natural History, Publication 386. Volume XVII, Number 1.*
By K. H. Rechinger, Jr. Field Museum

of Natural History, Chicago. $1.50. 9½ x 6¾; 151; 1937 (paper).

This catalogue of plants and drugs of Iran and Iraq makes interesting reading. The material was obtained by three groups of collectors during the past eight years from bazaars, markets, fields and gardens in the Near East. Information was obtained and is here recorded regarding the use of these plants and drugs (nearly 300 plants, 15 drugs of mineral origin and about the same number of animal origin) in the treatment of diseases and in prescriptions for various ailments. No new drug plants were found. This can hardly be considered strange, however, since the search for curative drugs and palliatives in the Iran and Iraq regions undoubtedly dates far back into prehistory. The report, which is without index, concludes with an alphabetical list of native plant names with Latin equivalents.

In the second of these publications of the Field Museum the author has attempted to bring up to date the classification of the North American species of *Rumex*. He has recorded 49 species and four hybrids. The most recent previous monograph on the subject mentions only 21 species. Besides detailed description of the species the author includes a key and a general discussion of *Axillares*.

La Botanique Canadienne a l'Époque de Jacques Cartier. *Contributions du Laboratoire de Botanique de l'Université de Montréal, No. 28.*

By Jacques Rousseau. Henry G. Fielder, 89 Chambers St., New York; Institut Botanique, Université de Montréal, Montréal; T. Oswald Weigel, Leipzig. 50 cents. 9 x 6¾; 86; 1937 (paper).

The three voyages of Jacques Cartier to the North American continent in the period 1534–1542 were of such importance as scientific expeditions that they have been studied from many aspects. The present paper emphasizes the botanical studies made by Cartier, particularly in Canada. Although Cartier was not a botanist, his observations concerning plant life in the new world are of such magnitude and precision that they merit considerable study. The purpose of the voyages was to study possibilities of colonization, and in consequence the writings concerning plant life assume an economic note. In describing the plants, Cartier always compares them with similar species familiar to everyone in France, and tells of what importance they would be to colonists, either as food, building materials, or as general agricultural necessities. Dr. Rousseau has included in this work a brief bibliography and record of activities of many of Cartier's contemporaries and predecessors. An annotated list of plants mentioned, an extensive bibliography, and an alphabetical index to plants conclude the text.

Die Wuchsstoffe der Pflanzen. *Ein Querschnitt durch die Wuchshormonforschung.*

By Gerhard Schlenker. J. F. Lehmanns, Munich and Berlin. 3.60 marks (paper); 4.50 marks (bound). 9¾ x 6½; 106; 1937.

The subject matter of this book was made possible by the discovery of growth substances in the *Avena* coleoptile. Subsequently much work has been done on these *Wuchsstoffe*, and many theories concerning them have been expounded. In this book *Wuchsstoffe A* (auxins) are considered especially with reference to methods of their qualitative and quantitative detection and to fundamental experiments performed on *Avena* coleoptiles. Auxin a, auxin b, and heteroauxin are discussed as to structural constitution, occurrence, and effect on phototropisms and geotropisms of stems and roots. Leaf movements, both epinastic and hyponastic, are also included in the discussion of the effect of growth substances in group A. In connection with phototropic phenomena in plants the Blaauw theory is discussed. The second group, *Wuchsstoffe B*, includes bios, substances that function much like vitamins, and vitamins which are considered as to their action on fungi and higher plants. The mechanism of translocation of plant-growth hormones is discussed. An excellent brief review of an important field.

A History of Agricultural Experimentation and Research in the United States 1607–1925. *Including A History of the United States Department of Agriculture. U. S. Department of Agriculture, Miscellaneous Publication No. 251.*

By Alfred C. True. U. S. Government Printing Office, Washington. 25 cents. 9 x 5¾; vi + 321; 1937.

A history of agricultural experimentation and research in the United States, when reduced to a volume of this size must necessarily be sketchy. However, brief though it is, one obtains from this general survey some idea of the early forces working toward the establishment of public agencies for agricultural research in this country. The movement in the states toward this end is traced from 1840 to 1875, and state agricultural research programs carried on without federal aid until 1888 are recorded in some detail. The history of the Department of Agriculture is traced from its organization in 1862 through the administrations of its six commissioners and eight secretaries of agriculture up to 1925. Following the passage of the Hatch Experiment Station Act of 1887 and the organization of the Department of Agriculture as of cabinet rank, the field of action of these agencies spread out broadly and only brief summaries are made of the principal undertakings of this period. Subject and name indices and a bibliography are included.

Plant Ecology.

By Hilda Drabble. Edward Arnold and Co., London; Longmans Green and Co., New York. $2.50. 8½ x 5½; 142 + 12 plates; 1937.

The present trend of studying plant ecology in close relation with plant physiology has been very well adapted by the author in preparing this elementary text. The volume is not intended as a substitute for a general botany text but rather to supplement it, and carry the student into the ultimately practical study of plants in relation to their environment and in relation to each other.

The book is made up of two fundamental divisions: plant physiology, which includes a detailed study of the soil and of the basic physiological processes in plants, and plant ecology, which describes plant communities. The text is well written in a style that emphasizes detail without the burden of technical terms. It is illustrated by 24 well selected photographs of typical plant communities. The volume contains a short bibliography, a list of test questions, and an index to plant names.

The Genus Youngia.

By Ernest B. Babcock and G. Ledyard Stebbins, Jr. Carnegie Institution of Washington, Washington, D. C. $1.25. 10 x 6⅞; 106 + 5 plates; 1937 (paper).

This genus was previously considered merely as one group in the genus *Crepis*. But as it did not seem to be closely related to the other species in that genus it has at various times been segregated as the genus *Youngia*. Until the present detailed study was made, however, no characters were known other than those typical of the genus *Crepis*. In this monograph the authors have disclosed a number of distinguishing characters, among which the cytological differences are the most striking.

The authors have produced a thorough analysis which definitely establishes the genus *Youngia*. They have included a history of the genus, criteria of classification, relationships to other genera, distribution, etc. as well as detailed descriptions and discussions of the various species and subspecies. Drawings, photographs, and tables illustrate the text.

The Physical Basis of Mycotrophy in Pinus. *Based on a thesis submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Division of Biology, Harvard University, and deposited with the Widener Library, Cambridge, Massachusetts, May 1, 1935. Black Rock Forest Bulletin No. 6.*

By A. B. Hatch. Black Rock Forest, Cornwall-on-the-Hudson, New York. $3.50 9 x 6; x + 168; 1937 (paper).

A work of much interest to silviculturists. After a review of the literature on the prevalence of the ectotrophic mycorrhizal habit, the factors responsible for variation in the abundance of mycorrhizae, and the theories of mycotrophy, the author discusses the methods now in use in investigating the mycorrhizal relationships and the results of various soil culture experiments. His work shows the inadequacy of the so-called organic nitrogen theory, the errors in the pathogenic theory, and strengthens and extends the so-called mineral salt theory first postulated by Stahl in 1900. The experiments also show that instead of retarding development mycorrhizal fungi stimulate root development and that "trees are dependent on symbiotic association with mycorrhizal fungi for several soil nutrients, and therefore for their existence, in all but the most fertile soils."

ABC OF AGROBIOLOGY. *The Quantitative Science of Plant Life and Plant Nutrition for Gardeners, Farmers and General Readers.*
By O. W. Willcox. W. W. Norton and Co., New York. $2.75. 8 x 5½; 323; 1937.

According to the author, agrobiology is a new science. It is the science that acquaints us with two scales: one to measure the vital energy residing in cultivated plants; the other to measure "the crop-producing values of the factors that enable plant life to expand." The book described these scales and the methods of derivation as well as enumerating the various growth factors involved. Then, having stated these "universal verities of nature," it shows how they are applicable to the main problems of farmers and gardeners. Though dealing with somewhat technical material, the book has been written in as simple language as possible so that not only serious students, but also the "gardeners, farmers and general readers" for whom it is primarily designed may benefit to the utmost. The text is clearly illustrated with tables, graphs, and diagrams, and is indexed.

A LEAF KEY TO FLORIDA BROAD-LEAVED TREES. *Native and Exotic, except Palms.*
By Mary F. Barrett. Illustrated by E. Bradley Tuttle. (Obtainable from M. F. Barrett, 57 Union Street, Montclair, N. J.). $1.00. 9¼ x 6⅛; 79; 1937 (paper).

Over 600 Florida species have been keyed in this booklet, most of which are readily found either growing wild or in parks or botanical gardens. The key has been made entirely on the basis of leaves. They are more abundant and are present a longer time in the year than either fruit or flowers. Eight plates of clear pen and ink drawings supplement the text. Although both botanical and common English names have been given in the detailed index, this is arranged alphabetically only by the botanical nomenclature and therefore offers rather a problem for the uninitiated.

A TEXTBOOK OF PLANT VIRUS DISEASES.
By Kenneth M. Smith. P. Blakiston's Son and Co., Philadelphia. $5.00. 8 x 5⅝; x + 615; 1937.

About 135 plant viruses are described as to their properties, modes of transmission, distribution, and diseases they cause in this comprehensive treatise of the subject. Viruses which are chiefly associated with a particular host plant are grouped together. For convenience, the host plants, together with a brief description of the symptoms of the disease and the virus causing it, are listed alphabetically at the end of the book. There is also a general index and indices of authors and viruses. This is an indispensable book for the plant pathologist.

BRITISH STEM- AND LEAF-FUNGI (COELOMYCETES). *A Contribution to Our Knowledge of the Fungi Imperfecti Belonging to the Sphaeropsidales and the Melanconiales. Volume II. Sphaeropsidales. Comprising Sphaerioideae, with Coloured Spores; Nectrioideae, Excipulaceae, and Leptostromataceae; and Melanconiales.*

By W. B. Grove. The University Press, Cambridge; Macmillan Co., New York. $6.00. $8\frac{1}{2} \times 5\frac{1}{2}$; xii + 407; 1937.

This completes the author's detailed morphological account of British fungi belonging to the orders Sphaeropsidales and Melanconiales. (Vol. I noticed in Q.R.B. Vol. II, No. 1.) Descriptions of all known species are given together with their geographical distribution, and numerous illustrations are included. The volume also contains the following indices and lists: Ascomycetes; Ascomycetes that have a discomycetous affinity; hosts; index of binomial names; and a list of the most important authorities' names which are usually abbreviated in citations. A basic reference source for the mycologist.

PRINCIPLES AND METHODS OF TREE-RING ANALYSIS. *Carnegie Institution of Washington Publication No. 486.*

By Waldo S. Glock with a foreword by A. E. Douglass and a contribution by G. A. Pearson. Carnegie Institution of Washington, Washington, D. C. $2.00 (paper); $2.50 (cloth). $10 \times 6\frac{7}{8}$; viii + 100 + 14 plates; 1937 (paper).

The work of Dr. A. E. Douglass of the University of Arizona, who began as an astronomer, becoming in succession a meteorologist, a botanist, and an archaeologist, needs no introduction. His book in two volumes describing his method of tree-ring identification and its use in dating the pueblos of the southwestern states has been out of print for many years, and this work by one of his associates fills a great need. Its numerous illustrations are beautifully executed, its bibliography and index each covers three pages, and it will be welcomed everywhere.

FOUNDATIONS OF SILVICULTURE UPON AN ECOLOGICAL BASIS. *Second Edition.*

By James W. Toumey and Clarence F. Korstian. John Wiley and Sons, New York; Chapman and Hall, London. $4.50. 9×6; xxi + 456; 1937.

In this important textbook the late Professor Toumey combined the researches of the forester and the ecologist into a substantial basis for the practice of American silviculture. In the present revised edition there has been a considerable rearrangement of the sections and the division of the book has been changed from two to three parts. Part I (10 chapters) deals with the environment of the forest; Part II (2 chapters) with the influence of the forest on its environment; and Part III (6 chapters) with the forest itself. The volume includes an appendix of common and technical names of trees, a useful bibliography of 32 pages, and an index.

PLANT LIFE FORMS.

By C. Raunkiaer. Translated by H. Gilbert-Carter. Oxford University Press, New York; Clarendon Press, Oxford. $2.00. $9\frac{3}{4} \times 6\frac{1}{4}$; vii + 104; 1937.

Although the original work of which this volume is a translation was written in 1907, it has not grown out of date, nor will it in all probability ever do so, because of the care and accuracy shown in the research upon which it is based. Like many other botanists, Raunkiaer early saw the necessity of the classification of plants into groups of finer distinction than simply trees, shrubs and herbs. In consequence, he set about the enormous task of classifying all plants according to their life economy, i.e., their adaptations to adverse conditions in their environment. His interest was those structural characteristics that reflect the essential relationships of plants to climate. No emphasis is placed upon mere structure as such. But particular structures are appraised as assets or liabilities to the plant in its struggle for existence. The text is abundantly and well illustrated. There is no index.

WEEDS, WEEDS, WEEDS.

By Sir Charles V. Boys. The Old Westminster Press, London. 1s. 2d. net. $8\frac{1}{2} \times 5\frac{1}{2}$; 69; 1937 (paper.)

This pamphlet by one of the grand old men of British science is not written with the detached impersonality of a scientific

botanist. It is a pleasant but very practical account of the author's own experiences with gardening, and is designed to help both the amateur and the professional gardener, but especially the former. Various common weeds such as plantains, dandelions, yarrow, and docks are considered. Each is taken up separately, described from the point of view of its nuisance-value, and a partial or complete cure prescribed. Since the information contained in the text is almost exclusively from the author's experience, it is obvious that all weeds cannot be included. It is delightful reading, besides being tough on weeds.

THE POTATO *Its Culture, Uses, History and Classification. Fourth Edition Revised.*

By William Stuart. J. B. Lippincott Co., Philadelphia. $3.00. $8\frac{1}{4}$ x $5\frac{1}{2}$; xv + 508 + 4 plates; 1937.

This fourth edition of a standard text, now well established as the classic in its field, has been extensively revised, since its last preceding appearance, in respect of six of its chapters. Otherwise it stands much as before except for minor changes to bring it up to date. We wish it the continued success that it merits.

PLANT SCIENCE MANUAL.

By Francis Ramaley. Obtainable from Francis Ramaley, Boulder, Colorado. 50 cents. 58; 9 x 6; 1937 (paper).

This is a pamphlet designed for use in a beginners course in botany. It gives specific directions for the laboratory work, leaving virtually nothing to the ingenuity of either teacher or student. The work covers the territory included in the usual elementary course.

A LIST OF MISSOURI FUNGI *with Special Reference to Plant Pathogens and Wood-Destroying Species. University of Missouri Studies, A Quarterly of Research, Volume XII, Number 3.*

By Willis E. Maneval. University of Missouri, Columbia. Single copies $1.25; annual subscription $4.00. $10\frac{1}{2}$ x $7\frac{1}{2}$; 150; 1937 (paper).

This pamphlet contains an up-to-date list of the fungi of Missouri, a host index, and an extensive bibliography.

BLACK ROCK FOREST PAPERS. *Vol. I, No. 10, The Relation Between Mycorrhizae and the Growth and Nutrient Absorption of Coniferous Seedlings in Nursery Beds*, by H. L. Mitchell, R. F. Finn and R. O. Rosendahl. *No. 11, The Effect of Soil Texture Upon the Growth of Red and Chestnut Oaks*, by Harold F. Scholz.

Black Rock Forest, Cornwall-on-the-Hudson, New York. 11 x $8\frac{1}{2}$; 23; 1937 (paper).

MORPHOLOGY

A METHOD OF ANATOMY. *Descriptive and Deductive.*

By. J. C. Boileau Grant. William Wood and Co., Baltimore. $6.00. 10 x 7; xx + 650; 1937.

"The book is meant to be a working instrument designed to make anatomy rational, interesting, and of direct application to the problems of medicine and surgery. The bare, dry, and unrelated facts of anatomy tend rapidly to disappear into forgetfulness. That is largely because its guiding principles are not grasped so as to capture the imagination."

Without doubt this book at least partially accomplishes this purpose. It is an admirably written text, especially insofar as it shows relationships within a particular body region. The entire volume is of regional rather than systemic organization, considering in turn the upper limb, abdomen, pelvis, lower limb, thorax, and head and neck. Special attention is given to function, and reference is often made to embryology and comparative anatomy.

The illustrations have been painstakingly made and justify Dr. Grant's pride in this feature. They consist of entirely simple line drawings showing relationships in terms of position, relative distances, and angles. However, what

has been a gain in one respect is a loss in another in that these drawings do not approach closely enough to actuality in structure. They might well be supplemented with a few plates. Whether or not the method used helps to capture the imagination, some of the facts of anatomy have been adorned and possibly made more succulent.

THE DEVELOPMENT OF THE VERTEBRATE SKULL.

By G. R. de Beer. Oxford University Press, New York; Clarendon Press, Oxford. $10.00 $9\frac{3}{4} \times 6\frac{1}{2}$; xiv + 552 + 143 plates; 1937.

After having had the privilege of researching upon the embryological development of all the groups of vertebrates, the author is exceptionally well qualified to write a book of this type. The development of the vertebrate skull has long been studied in an attempt to delve more deeply into the phylogeny of the vertebrate types, but the results of previous studies have been so lacking in coordination and confusing in nomenclature that there has been a real need for a well organized and systematic view of the subject. This book is an attempt to meet this need.

In his introduction de Beer discusses the historical aspects of the subject, the segmentation of the head, and the nature of the tissues involved in skull formation. In the text proper he treats in a comparative manner the problems of morphology, growth, phylogenetic affinity, and experimental morphogenesis. The discussions are very detailed, but the very nature of the work requires this since, in the words of the author, "... in morphology, general principles are founded on matters of quite intricate detail ...." The volume concludes with an annotated agenda, a complete bibliography, and indices of subjects and of genera. Included also are some 140 plates of well-labeled drawings and diagrams of the skull development of the different vertebrate types.

The book is not intended to be used as a text, nor does the author expect anyone to read it from cover to cover; yet it will be extremely valuable as a reference to zoologists in general, anatomists, both comparative and human, embryologists, and palaeontologists.

THE MICROTOMIST'S VADE-MECUM (BOLLES LEE). *A Handbook of the Methods of Animal and Plant Microscopic Anatomy. Tenth Edition.*

Edited by J. Brontë Gatenby and Theophilus S. Painter with the collaboration of D. G. Catcheside, Harold J. Conn, E. S. Duthie, Helen Pixell-Goodrich, J. G. Greenfield, W. W. Kay, Reginald Ludford, K. C. Richardson, Ruby O. Stern and Raymond Whitehead. P. Blakiston's Son and Co., Philadelphia. $9.00. $8\frac{3}{4} \times 5\frac{1}{2}$; xi + 784; 1937.

The first edition of this extremely useful guide to laboratory technique appeared 53 years ago. The author, Arthur Bolles Lee (1849–1927), just missed seeing the birth of the ninth edition which appeared in 1928 (Q. R. B., Vol. 4. No. 1). An Englishman, Lee spent most of his life in Switzerland. He is said personally to have tried out every method and technique that appeared in the earlier editions of the *Vade-Mecum*. Several chapters still stand almost as he wrote them. The present editors have had a group of ten collaborators, all connected with British institutions with the exception of H. J. Conn, of the New York Agricultural Experiment Station, Geneva, N. Y., and a number of assistants to aid them—but no longer is it possible to test out all of the various techniques. A number of changes have been made in the present edition: Two sections of the ninth edition have been dropped and a section on plant technique substituted; there are new chapters on frozen section technique and on vital staining; the chapter on staining has been recast as has the one on blood and glands; the chapter on fats has been completely rewritten; and the section on the nervous system has been revised. The index covers 43 pages. Plainly there is still life in the good old Biologist's Bible of our youth.

FUNDAMENTALS OF ANATOMY.

By Carl C Francis. C. V. Mosby Co., St. Louis. $2.75. 9 x 6; 320; 1937.

Details of structure have been omitted in this work, though the essential features of anatomy are clearly presented and excellently illustrated, most of the cuts being entirely new. Following a discussion of cells and tissues, the author continues with the structure of the organ systems and special senses. A fine chapter is that on surface anatomy. The conciseness of the material makes the book a little impractical as a text for medical students, but due to this very feature, along with a very complete glossary and index, it will serve well as an anatomical guide.

DIE LUNGENVENEN DER WIRBELTIERE. *Besonders der Säugetiere und des Menschen. Lunds Universitets Årsskrift. N. F. Avd. 2, Bd. 33, Nr. 6.*

By Gaston Backman. C. W. K. Gleerup, Lund; Otto Harrassowitz, Leipzig. Kr. 7. $10\frac{1}{4}$ x $7\frac{3}{4}$; 112; 1937 (paper).

One sees in this publication the results of a careful study of the bronchial veins. The author gives his method of preparation and a short history of work done in this field. The primates, ungulates, carnivores, and rodents occupy the greater part of the book, with briefer accounts for other mammals and the lower vertebrates. There are numerous drawings and a list of literature. According to the relationships among the trunks and branches of the veins, arteries, and bronchi, the material is summarized and divided into four general types.

PHYSIOLOGY AND PATHOLOGY

MEDICO-LEGAL ASPECTS OF THE RUXTON CASE.

By John Glaister and James C. Brash. William Wood and Co., Baltimore. $6.00. $9\frac{3}{4}$ x $7\frac{1}{8}$; xvi + 384; 1937.

In September 1935 scattered portions of human remains were found in a stream bed in Dumfriesshire, Scotland. The introduction of this book goes briefly into the history of this now famous murder case in which a Dr. Ruxton not only murdered his wife and nursemaid, but mutilated and dissected their bodies in an attempt to make them unidentifiable. The bulk of this volume is a long and detailed description of the intricate work done by the members of the faculties of the Universities of Glasgow and Edinburgh in piecing together the remains and helping to identify the bodies.

According to the authors this volume was prepared in response to requests from officers of police forces and from members of the medical and legal professions and is "intended for those professionally interested in methods employed in the preparation of medical and other technical evidence." Among the 172 vivid illustrations some of the slightly revolting photographs of disintegrated and mutilated portions of the bodies show in the clearest manner how what is perhaps the most skilful job of biological detection on record was done. This is an exceptionally well written account with a thorough and complete summation of every conceivable angle of the investigation and of every clue that eventually helped to identify the victims. The sixteen chapters are supplemented by appendixes and there is an excellent index.

NUTRITION. *Final Report of the Mixed Committee of the League of Nations on the Relation of Nutrition to Health, Agriculture and Economic Policy.*

League of Nations. Columbia University Press, New York. $2.00. $9\frac{1}{2}$ x $6\frac{1}{4}$; 327; 1937 (paper).

The League of Nations Mixed Committee on the Problem of Nutrition was organized in 1935 to make a thorough investigation of food consumption in Northern and Western Europe and in America and its relation to economic and agricultural conditions. This final report is divided into three parts, the third being a detailed presentation of the results of the investigation while the first and second are respectively a general summary of the findings and an exposition of the nutritive value of the more important foods. In the

main this investigation showed that in the last two decades the trend in food consumption has been towards a better and more diversified diet. In particular there has been a decrease in the use of such staples as wheat and a corresponding increase in the consumption of dairy products, fruits and vegetables. This progress, however, has not been sufficient to wipe out malnutrition which is still very much in evidence in a number of countries. Therefore, the Committee recommends that agriculture be allowed greater freedom to meet the changes in consumption demands and that governments should develop economic programs which will permit a more adequate satisfaction of the dietary needs of the population. As all similar publications of the League this will be found useful for the references and data which it contains.

Einführung in die vergleichende biologische Anatomie der Wirbeltiere. *Zweiter Band. Biologische Anatomie der Ernährung.*

By Hans Böker. Gustav Fischer Verlag, Jena. RM. 13.50 (paper); RM. 15. (cloth). 10 x $6\frac{3}{4}$; xi + 258; 1937.

This is the second volume of an extremely important book. The first volume has already been noticed in this Review (Volume 11, No. 3, pp. 357–358.) This second volume deals with the biological anatomy of nutrition. There will appear in the future still another volume dealing with the anatomy of reproduction and environmental adjustments.

The author divides his discussion of the anatomical adaptations pertinent to nutrition under the following heads: 1, Searching for and recognizing food, both actively and passively; 2, Acquiring food; 3, Swallowing and breaking up food both mechanically and chemically; 4, Absorbing food, and the increase of body weight; 5, Excretion of waste products; and 6, Maintaining of the equilibrium of metabolism through respiration, hormones, and the influence of the nervous system. Under each of the sections he discusses fishes, amphibians, reptiles and birds, mammals and man. Much of the discussion is drawn from his own observations and therefore the book is more interesting and less textbookish than most discussions of comparative anatomy. Böker is a real pioneer, opening out new fields in anatomy. The only reason that English translations of the successive volumes of this masterly work are not available is because American publishers are not very bright.

The Machinery of the Body.

By Anton J. Carlson and Victor Johnson. University of Chicago Press, Chicago. $4.00. 9 x $6\frac{1}{4}$; xvii + 580; 1937.

This elementary text was published especially to meet the needs of students in freshmen physiology courses, but the breadth of material presented and the semi-popular style in which it is written are indicative of its value as a reference for the more advanced student of physiology, and as a guide for the energetic layman in his search for more knowledge of the human body and its functioning. The authors have refrained from the stereotyped technical discussions of theories and processes which so often characterize physiology texts, and have presented a series of thoroughly checked observations from carefully controlled experiments.

The book is organized on the organ-system plan, beginning with a discussion of the cell as the unit of structure and function and ranging through the circulatory, respiratory, digestive, excretory, nervous, and glandular systems of the body. The chapters on the glands of internal secretion and on the body defences against disease present much of the latest experimental work in physiology. A wealth of illustrative material both photographic and hand drawn is included in each chapter.

A detailed table of contents, a list of selected references, and a lengthy index complete the text.

Clinical Parasitology.

By Charles F. Craig and Ernest C. Faust.

Lea and Febiger, Philadelphia. $8.50. $9\frac{1}{4} \times 5\frac{7}{8}$; 733; 1937.

Members of the medical profession will find this an excellent aid in their practice. The volume is concerned with protozoan and metazoan parasites, presenting clinical and laboratory methods of diagnosis, type of damage produced, source and path of infection, approved therapeusis, and methods of community control. A growing knowledge of the seriousness of animal parasites in their human and economic toll points to the need for recent and accurate information, especially by physicians and health officers.

The introduction is in itself a fine essay on general parasitology. Subsequent chapters deal with the several classes of protozoa; the nematodes, flukes, and flatworms; and finally with the arthropods as parasites themselves and as vectors. Numerous illustrations show the forms of the parasites and the physical manifestations of the diseases, with maps to show geographical distribution. The book can be used as a text or reference for students, since it contains a technical appendix regarding the collection and preparation of representative forms, as well as a lengthy bibliography, and index of subjects and of authors. The volume may be recommended for the thorough treatment which the authors have accorded their subject.

TWENTY-FIVE YEARS OF HEALTH PROGRESS. *A Study of the Mortality Experience Among the Industrial Policyholders of the Metropolitan Life Insurance Company 1911 to 1935.*

By Louis I. Dublin and Alfred J. Lotka. Metropolitan Life Insurance Co., New York. $9\frac{1}{4} \times 5\frac{7}{8}$; xi + 611; 1937.

This book analyzes the mortality experience of the Industrial policy-holders of the Metropolitan Life Insurance Company from 1911 to 1935. As the authors point out, these insurance data do more than merely supplement the mortality statistics of the Bureau of the Census, since in 1911 the Death Registration Area comprised only ten states and not until 1933 was the whole United States included In the twenty-five years there has been an average annual decline in the mortality of the Metropolitan Life Insurance Company's Industrial policyholders, standardized for color, sex and age, of 1.3 per cent of the average rate during the period. The expectation of life at birth has increased from 46.63 years in 1911–12 to 60.25 years in 1935. Besides their consideration of mortality as a whole, the authors analyze the deaths from the more important separate causes. This is a book which will repay careful study by every person interested in the health of the community.

PHENOMENON OF LOCAL TISSUE REACTIVITY *and Its Immunological, Pathological and Clinical Significance.*

By Gregory Shwartzman. Foreword by Jules Bordet. Paul B. Hoeber, Medical Book Dept. of Harper and Bros., New York. $7.50. $9\frac{1}{4} \times 6\frac{1}{8}$; xxviii + 461; 1937.

As Bordet says in the foreword, after sixty years and more of intense study of the varied reactions of sensitized living tissues to bacterial toxins, no one would have believed that a remarkable phenomenon still remained to be recognized and studied, but there it was. The striking hemorrhagic lesions produced by Shwartzman must have been seen many times in the past, but their mode of production was not understood until he made his extensive studies. The phenomenon which now goes by his name appears when certain substances, usually the products elaborated by some microörganisms, enter the circulation several hours after they have been used to sensitize a certain area, usually in skin or mucosa. Curiously, the second dose of the toxin must be given intravenously, and preferably about twenty-four hours after the first dose.

The book is most thought-producing, and no one who deals with the reactions of the body to invasion by bacteria or cancerous cells can afford to miss reading it.

ASSESSMENT OF RISKS IN LIFE ASSURANCE PRACTICE.

By Jehangir J. Cursetji. (Obtainable

from Jehangir J. Cursetji, Bombay). 7½ x 4⅝; 18; 1937 (paper).

This pamphlet contains within its few pages a very considerable amount of information which should be useful to medical examiners for life insurance companies. Dr. Cursetji shows very clearly that his long years of service together with intelligent observation have enabled him thoroughly to appraise the effects of personal and family history, general health, environment, occupation, etc. upon the insuring risk of the individual. Although he builds his work primarily upon conditions effecting life and health in India, the results of his observations apply in all climates and countries.

LIPOGENESIS IN THE ANIMAL BODY, WITH SPECIAL REFERENCE TO THE PHYSIOLOGY OF THE GOOSE. *Carnegie Institution of Washington Publication No. 489.*

By Francis G. Benedict and Robert C. Lee. Carnegie Institution of Washington, Washington, D. C. $2.00 (paper); $2.50 (cloth). 10 x 6¾; ix + 232; 1937.

The deposition of fats transformed from carbohydrates is the problem pursued in this detailed study of the physiology of the goose under normal circumstances and after starvation and over-feeding. During lipogenesis the respiratory quotient is much higher than during combustion of pure carbohydrate, and it was found that the quotient was high after surfeit feeding, an indication that carbohydrate was being deposited.

GENERAL HYGIENE AND PREVENTIVE MEDICINE. *A Text-Book for College Students, Medical Students, Nurses, Public Health Workers and Social Workers.*

By John Weinzirl. Edited by Adolph Weinzirl. Lea and Febiger, Philadelphia. $4.00 9¼ x 5⅞; 424; 1937.

The author of this book, a bacteriologist interested in public health, has approached his subject in what he feels is a new way, stressing epidemiology and methods of prevention of disease. Statements as to treatment are brief, and often only long enough to show the student how little can be done in a curative way once the disease has attacked. After discussing methods of immunization and the use of specifics such as medicines and vitamins, the writer takes up the control of carriers and of groups in the community, and of the several sanitary, physical, personal, and social environments.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 464. Abt. V. Methoden zum Studium der Funktionen der einzelnen Organe des tierischen Organismus, Teil 7, Heft 13 (Schluss). Gehörsinn, Hautsinne, Kraftsinn (Muskelsinne), Geschmack- und Geruchsinn, Statischer Sinn, Stimme und Sprache.* Containing following articles: *Neue Verfahren zur Erforschung der Leistungen des Druck-, Temperatur-, Schmerz- und Kraftsinnes (Schluss),* by Emil von Skramlik; *Verfahren zur Prüfung der haptischen Leistungen,* by Emil von Skramlik; *Methoden zur Messung von Schall und Erschütterungen,* by Hermann Reiher.

Urban und Schwarzenberg, Berlin. RM. 13.50. 10 x 7; 236; 1937 (paper).

The first paper in this volume is the continuation and conclusion of a paper on the function of perceptors of pressure, temperature, pain, and kinesthesia published in *Lieferung* 463, and already noticed in the Q. R. B. (Vol. 12, p. 492). There is also an article on a method for testing tactile function, and a new method described for testing the range of perception in man of sound vibrations—those that can be borne with ease as well as those that are harmful or painful.

MATERNAL DEATHS—THE WAYS TO PREVENTION.

By Iago Galdston. Commonwealth Fund, New York; Oxford University Press, London. 75 cents (cloth); 50 cents (paper); 25 per cent discount on lots of 10 or more. 8¼ x 5½; [10] + 115; 1937.

This booklet written for the general public is based essentially on the facts brought out in the survey of maternal mortality conducted in New York City a few years ago. In a style both limpid and concise Dr. Galdston considers the

practical means of reducing puerperal mortality. He concludes that the problem should be attacked by educating the general public and by developing better medical standards. In an appendix is contained an outline of the community organization of obstetrical services in the hospitals of Cleveland, Ohio.

INTRODUCTION TO PHYSIOLOGICAL OPTICS.
By James P. C. Southall. Oxford University Press, New York and London. $5.50. $9\frac{1}{4}$ x 6; x + 426 + 3 plates; 1937.

This is a simple and concise exposition of the fundamental facts and theories regarding the optical system and the physiology of the eye. The author describes the anatomy and kinematics of the eye, optical defects and means of correction, binocular vision, color vision and colorimetry and certain problems of temporal and spatial reactions of the organ of vision. The intent of the book is to give a clear understanding of the subject to undergraduate students and laymen, and the author has certainly achieved his objective.

THE FUNCTION OF THE SUB-OCCIPITAL MUSCLES. *The Key to Posture, Use and Functioning.*
By A. Murdoch. (Obtainable from A. Murdoch, Bexhill-on-Sea, Sussex, London). $8\frac{1}{2}$ x $5\frac{1}{2}$; 19; 1937 (paper).

Mr. Murdock believes that the seat of good posture lies in the control of the "Cranial Globe" through the action of the sub-occipital muscles. He attempts to provide an anatomical explanation for Alexander's theory of "The Primary Control," noticed in this Review several years ago (Vol. 8, p. 241). The brochure suffers from a lack of illustrative material.

BIOCHEMISTRY

THE BIOCHEMISTRY OF CELLULOSE, THE POLYURONIDES, LIGNIN, ETC.
By A. G. Norman. Oxford University Press, New York; Clarendon Press, Oxford. $5.00. $9\frac{1}{4}$ x $6\frac{1}{4}$; viii + 232; 1937.

In both industry and agriculture a large part of the raw material in the form of plant derivatives has been going to waste. With further advancement in the biochemistry of the complex substances that make up cell walls, it is probable that considerable savings could be accomplished through new and useful outlets for these materials. The author describes to the full extent of present knowledge, the properties, composition and structure, preparation, and biological decomposition of cellulose, the hemicelluloses, pectins, gums, mucilages, and lignin. He then critically surveys the theories of cell-wall metabolism. Bacterial and fungal polysaccharides are also considered. This book will be a standard reference for anyone interested in plant derivatives. It is indexed by authors and subjects.

PERSPECTIVES IN BIOCHEMISTRY. *Thirty-one Essays presented to Sir Frederick Gowland Hopkins by past and present members of his Laboratory.*
Edited by Joseph Needham and David E. Green. University Press, Cambridge; The Macmillan Company, New York. $4.75. $8\frac{1}{2}$ x $5\frac{1}{2}$; x + 361 + 6 plates; 1937.

An interesting and stimulating volume. The aim of the writers (there were 31 contributors) has been

> to indicate the most promising lines of advance in the various fields which they survey, and while maintaining a due standard of criticism, to speculate a little on the likely paths of future thought and discovery. It will be seen that in accordance with the wide interests of the founder and head of the Cambridge Biochemical Laboratory, the essays touch on many aspects of the science of life—physiology and zoology, embryology and genetics, medicine, bacteriology, and nutrition—with all its great bearing on human welfare.

It is only possible in these columns to mention a few of the subjects discussed and the authors—Proteins and cell-organization by R. A. Peters; Chemical aspects of morphogenetic fields by Joseph Needham; The chemical regulation of insect growth by V. B. Wigglesworth; The biological function of magnesium by Ida Smedley Maclean; Calcium and blood

coagulation by John Mellanby; and Vitamin C and infection by Leslie J. Harris.

CHEMISTRY OF THE BRAIN.

By Irvine H. Page. Charles C Thomas, Springfield, Ill. $7.50. 10 x 6⅝; xvii + 444; 1937.

This book was designed to bring together all the available data on the chemical constitution of the brain and the relation between chemical changes and certain pathologic processes in man especially. Following a brief historical note on the subject, the author proceeds to consider in some detail sterols, phosphatides, cerebrosides, carbohydrates, enzymes and vitamins. He discusses also the metabolism of the brain and its measurement; and in particular the effects of variation in water, fatty acid and nitrogenous metabolism. In addition, there are chapters on electrolytes and gases and the chemical changes associated with the growth of the brain. J. H. Quastel contributes a chapter on oxidations in the brain. In the treatment of each subject the author describes the chemical structures, and reviews the results of animal experimentations and of observations on human material. He does not limit himself to the brain only but discusses other organs as well. It is a thorough piece of work and the literature has been carefully selected.

VITAMIN CONTENT OF FOODS. *A Summary of the Chemistry of Vitamins, Units of Measurement, Quantitative Aspects in Human Nutrition and Occurrence in Foods. U. S. Department of Agriculture. Miscellaneous Publication No. 275.*

By Esther P. Daniel and Hazel E. Munsell. Government Printing Office, Washington. 15 cents. 10 x 6¾; 175; 1937 (paper).

At first glance this discussion of the vitamin content of foods seems far too technical for the layman. Nevertheless a little study shows that much that is of value to the housewife (one of whose chief duties is to have a knowledge of food values) can be gleaned from these pages. With over 100 pages of tabular matter and a bibliography of 771 titles, a useful reference work has been provided for both investigator and layman—and all for the very small sum of 15 cents.

ALLGEMEINE PHOTOCHEMIE. *Ein Hand- und Lehrbuch für Studium und Forschung für Mediziner, Biologen, Agrikultur-Chemiker, Botaniker, usw. Zweite, umgearbeitete und erweiterte Auflage.*

By J. Plotnikow. Walter de Gruyter and Co., Berlin and Leipzig. RM. 28.50. 9½ x 6½; viii + 909; 1936 (paper).

This second edition is a considerable enlargement over the first which appeared in 1910. There are new chapters on fluorescence, phosphorescence, light reactions, animal luminescence, the effects of ultra-red, ultra-violet and Röntgen rays. There is also a discussion of the place of photochemistry in other sciences. The book is an elaborate and thorough treatise on the subject, already a standard classic in the field.

TRAITÉ DE CHIMIE ORGANIQUE. *Tome V. Les Métaux en Chimie Organique. Alcools dans Toutes les Séries. Formation des Alcools par Voie Biochimique. Synthèse du Méthanol. Industrie de l'Alcoolé Thylique. Éthers-Oxydes. Éthers-Sels des Acides Minéraux.*

By P. Baud, Ch. Courtot, Cl. Fromageot, J. Lichtenberger, Ch. Prévost, J. B. Senderens. Published under the direction of V. Grignard, G. Dupont, R. Locquin and Paul Baud. Masson et Cie, Paris. 310 francs. 10 x 6½; xix + 1047; 1937.

As in earlier volumes of this comprehensive treatise, noticed from time to time in these columns, this weighty tome contains chapters by authorities in specialized sub-division of organic chemistry as indicated in the title. There are indexes of authors and substances, and a bibliography, up to January 1934, on physical constants of ethers of the mineral acids and monoalcohols. The *Traité* is a work of first rank importance.

SEX

GENITAL ABNORMALITIES, HERMAPHRODITISM AND RELATED ADRENAL DISEASES.
By Hugh H. Young. Drawings by William P. Didusch. Williams and Wilkins Co., Baltimore. $10.00. 10 x 7; xli + 649; 1937.

The subject of hermaphroditism and of genital abnormalities in general has very rarely received such a comprehensive treatment as in this volume. In the first chapter Young relates the myth of Hermaphrodites and the manifestations of art and literature based upon it. In the next two chapters, the development of hermaphroditism and the several varieties of hermaphrodites are described. The following chapters contain detailed case histories of pseudo-hermaphrodites and of the one case of true hermaphroditism seen by the author. In relation to this, there are also summarized the other 19 authentic cases reported in the literature. The remaining chapters include a discussion of, and case reports illustrating, the adreno-genital syndrome, ovarian tumors, hypergenitalism, hypogenitalism, gynecomastia, hypospadias, epispadias, cryptorchidism and the various other abnormalities of the genital system. The case reports contain not only the anamnesis and the results of the physical examination but whenever possible the histologic and pathologic findings in addition to a minute and profusely illustrated description of the surgical techniques used.

This rapid survey of its contents is sufficient to indicate that this book is of interest not only to physicians and surgeons but to all students of biology. It is well written and represents an important contribution to human biology as well as to surgery. Praise should also be given the artist and the publishers, whose efforts have made this a beautiful book.

CLINICAL CONTRACEPTION. *Second Edition.*
By Gladys M. Cox. Introduction by Lord Horder of Ashford. William Heinemann (Medical Books), London. 7s. 6d. net. $8\frac{1}{2}$ x $5\frac{1}{2}$; ix + 196; 1937.

PRACTICAL BIRTH CONTROL. *A Guide to Medically Approved Methods for the Married.*
By Rita Irwin and Clementina Paolone. Robert M. McBride and Co., New York. $1.75 $7\frac{1}{2}$ x $5\frac{1}{4}$; xxiii + 172; 1937

CALENDAR LOVE. *The Truth About Birth Control.*
By Herman Goodman. Hermes Publishing Co., New York. 50 cents. 8 x $5\frac{1}{2}$; 55; 1937 (paper).

The first two of these books outline the approved methods of contraception, their advantages and objections both from the effective and aesthetic points of view. Both are clearly written, straightforward, and adequately adapted to their respective purposes. Dr. Cox's book, written for physicians, contains also some statistical material regarding the effectiveness of contraceptive practice as obtained from clinics in England, a list of manufacturers of tested appliances, and illustrations. *Practical Birth Control* is intended for lay instruction.

Calendar Love, discussing other methods of birth control besides the use of the "safe-period," is stated in the preface to be "a discussion for adults old enough to recognize that storks don't bring babies, and that passionate kissing doesn't cause pregnancy." We make objection to that statement and suggest the substitution of the words "not intelligent enough" for "old enough." A sample sentence is: "If a man were supposed to wear one [a pessary], he never would."

WHAT'S BEHIND DIVORCE *or the Key to Happier Marriages. Based on an Analysis of Over One Thousand One Hundred Actual Divorces—Over Two Thousand Unhappy But Undivorced Marriages—Close to Twenty Thousand Years of Married Life.*
By John A. Hadaller. Obtainable from Major John A. Hadaller, San Bernardino, Cal. 75 cents. $7\frac{3}{4}$ x $5\frac{1}{2}$; 82; 1937 (paper).

The author, a Major and a member of the California State Bar Association, here offers a solution to the divorce problem so simple that it should solve the difficulty once and for all—provided, of course, that human nature changes. All hinges on the

interval in days between the birth dates between the prospective partners. About half the combinations on an 180 degree arc (approximately half a year) are rough on marriage and the other half are good in the absence of unfavorable elements such as drunkenness, troublesome fathers-in-law, or "general cussedness." On page 80 we read:

> Being a lawyer and a student of social matters, I know how difficult it is to change the established order of things. But I have faith. Louis Pasteur . . . finally forced the scientific world to accept what his patience and study had proved to him to be true about bacterial diseases. . . . If we are willing to make laws about vaccination for disease, I feel confident that we will also agree to make laws about this bacillus matrimonii. All we need is an interested body to espouse the cause and bring it forcefully to the attention of the legislatures.

We nominate the Major for a Nobel Peace Prize, or failing that a leather medal, for this great contribution.

Coming into Being among the Australian Aborigines. *A Study of the Procreative Beliefs of the Native Tribes of Australia.*

By M. F. Ashley-Montagu. Foreword by B. Malinowski. George Routledge and Sons, London. 21s. net. 10 x 6⅛; xxxvi + 362 + 5 plates; 1937.

Since Spencer and Gillen's report in 1899 ethnologists have debated whether the purported ignorance of the Australian aborigines regarding the relation between coitus and pregnancy is real or only apparent. In this somewhat massive book the author reexamines all the pertinent accounts dealing with the beliefs of the Arunta, in particular, regarding the causes of reproduction. From his analysis he is led to conclude that the Australian has no conception of the connection between coitus and pregnancy, and therefore paternity for him has no physiologic significance. Moreover, Ashley-Montagu contends, that in maintaining such a belief the Australian is consistent with his experience and with the structure of his social and religious ideas. In order to arrive at such a definitive and unqualified conclusion the author has had to generalize from evidence that at best is rather weak. This will undoubtedly lay him open to severe criticism. Nevertheless, it cannot be denied that the author has done a useful service in ordering the literature on the subject. The student will find the references handy, although an awkward and repetitious style in addition to some minor typographical errors make this book rather heavy reading. Malinowski's foreword is laudatory.

Rhythmic Diurnal Variations in the Oestrous Phenomena of the Rat and Their Susceptibility to Light and Dark. *Det Kgl. Danske Videnskabernes Selskab. Biologiske Meddelelser.* XIII, 7.

By Axel M. Hemmingsen and Niels B. Krarup. Levin and Munksgaard, Copenhagen. Kr. 3.00. 9½ x 6; 61 + 4 folding charts; 1937 (paper).

The Production of Mating Instincts in the Rat with Chemically Well-Defined Oestrogenic Compounds. *Det Kgl. Danske Videnskabernes Selskab. Biologiske Meddelelser.* XIII, 8.

By Axel M. Hemmingsen and Niels B. Krarup. Levin and Munksgaard, Copenhagen. Kr. .50. 9½ x 6; 9; 1937 (paper).

The first of these studies is concerned with the study of the diurnal rhythm of biological processes with stress on the daily periodicity of oestrous phenomena in the rat. Three groups of oestrous phenomena are studied: spontaneous muscular activity, mating instincts, and epithelial changes in vaginal smears of the rat under the normal day-night alternations. All three phenomena are found to be confined to certain hours of the day-night rhythm, heat and activity being greatest in the dark. The three phenomena can be shifted twelve hours by an establishment of an artificial day-night rhythm. The authors describe a convenient technique of artificial illumination.

The second study describes the results on spayed female rats with oestrogenic compounds under reversed conditions of day and night. In addition to positive vaginal smear changes, reliable mating instincts of high degree, and enhanced spontaneous muscular contractions were

produced. The question of specificity of the female sex hormones is briefly considered.

Love, Marriage and Divorce.

By Macpherson Lawrie. Methuen and Co., London. 5s. net. $7\frac{3}{8} \times 4\frac{7}{8}$; [8] + 198; 1937.

The two imaginary characters, Antony, aged forty-two, and Cathrine, aged thirty, are divorced after six years of married life. And why? Antony was weaned too early; was fed on processed foods that were not good for his system; had resort to medicinal correctives; developed badly adjusted social relationships with an inferiority complex as the consequence; was not quite all there so far as sexual attitudes were concerned, and so on. Cathrine was weaned and nurtured properly, but was thoroughly spoiled in the course of her up-bringing, and so on once more. If solemn head shakings over the undesirability of processed foods, the unnaturalness of this, and the inadequacies of that constitute scientific arguments, then this book is sane, discriminating, and full of good sense.

BIOMETRY

An Introduction to the Theory of Statistics. *Eleventh Edition, Revised Throughout and Re-Set.*

By G. Udny Yule and M. G. Kendall. Charles Griffin and Co., London. 21s. net. $8\frac{3}{4} \times 6$; xiii + 570 + 4 folding plates; 1937.

For a quarter of a century Yule's *Introduction* has been one of the best of the statistical textbooks. In the skill with which statistical methods have been presented for students with no greater knowledge of mathematics than algebra and the elements of coördinate geometry, and above all in its emphasis on pitfalls of erroneous interpretation into which the beginner—and often times the more advanced statistician—may be led it has been a model of what an introduction to statistics should be.

In successive editions new developments in the field of statistical methodology have been treated in supplements. With the present edition, however, it was decided that the time had come for a general revision, which has been made largely by Mr. Kendall. Old friends of the book will still find most of the material appearing in earlier editions, but several new chapters have been added on sampling, including an introduction to small sample methods. Other additions deal with moments, measures of skewness and kurtosis, simple curve fitting by the method of least squares, and interpolation and graduation. Another new feature is an appendix of tables of the area and ordinates of the normal curve, of the χ^2 integral for one degree of freedom, of Student's t distribution, and of Fisher's z distribution, and a nomogram of χ^2.

In concluding the chapter on interpolation and graduation Mr. Yule reminds the student that "*interpolare* means not only 'to polish up' (*polire*, to polish)—so that graduation is really the implication of the word—but hence 'to corrupt, to falsify.' It will do him no harm to bear this etymological meaning in mind, and keep a look-out accordingly." This caution may well apply not only to interpolation but to the whole practice of statistics.

The Methods of Statistics. *An Introduction Mainly for Experimentalists. Second Edition Revised and Enlarged.*

By L. H. C. Tippett. Williams and Norgate, London. 15s. net. $8\frac{1}{2} \times 5\frac{1}{2}$; 280; 1937.

This edition of Tippett's book, with but few minor exceptions, follows very closely the plan of the earlier one. The first three chapters have been revised and rewritten in such a manner as to lay a more thorough and substantial foundation upon which to develop succeeding theories and discussions. As in the former edition, emphasis has been placed upon the elementary concepts of the various theories of statistics, and where it has been possible to give proofs the more typical ones have been presented The logic of organization and presentation, and the clarity of style are characteristics which tend

to make the book popular among those workers whose mathematical backgrounds have been slighted. As in the former edition, numerous drawings, charts, tables and formulae are used as illustrative material. The list of bibliographic references and the index have been revised and enlarged.

PSYCHOLOGY AND BEHAVIOR

SCIENCE AND MUSIC.

By Sir James Jeans. The Macmillan Co., New York, University Press, Cambridge. \$2.75. $8\frac{1}{2} \times 5\frac{5}{8}$; x + 258 + 10 plates; 1937.

In some of the English universities it is the custom for the chief of the department of music to be also a member of the staff of the department of astronomy. The origin of the tradition is unknown, but since it is the tradition we need not be surprised when the best known English astronomer writes a book dealing with the scientific aspects of music.

Jeans is blest with clarity of expression surpassing that of most writers on any subject, and just as his astronomical writings are more easily comprehended than those of most of his contemporaries, his book on music makes better reading than the older works of Pole and Taylor whom he seems likely to supplant. He has used the excellent if old idea of blowing smoke through organ pipes while playing that instrument in order to make visible the aerial whirlpools that form where the jet impinges on the lip, and the resulting photographs are remarkable. The reader is likely to wish that Jeans had had a set of orchestral wind instruments of glass that might have been photographed under similar conditions. There is also a discussion of the sounds produced by the wind when it whistles in a chimney or in the rigging of a sail boat. The designer of aeroplanes would profit by reading this chapter, for he might learn more about what happens when the edge of a plane is turned into the wind. The discussion and comparison of the four systems of temperament is well handled and worth attentive study. There are also chapters on the acoustics of concert halls, and the anatomy and function of the human ear, and an index of six pages.

Of course, those who enjoy looking for defects will not be disappointed. Even Homer sometimes nods. Among these is the tendency to use well established terms with new and unfamiliar meanings. The author tells us that tunning forks have no natural harmonics (?) but that artificial harmonics may be produced by striking the fork on the end instead of on the side, and that the notes so produced are called clang-tones. Now the term clang-tone was originally used by Tyndall to designate a fundamental sounding with its natural harmonics—the equivalent of the German *Klangfarbe*. And the term artificial harmonic has been used in violin playing to denote merely a natural harmonic on a stopped string. Also Jeans tells us that the characteristic tone of the clarinet is due to the absence of the even partials, and that this is the result of the cylindrical bore of the instrument. But he does not explain why these same even partials are present in the synthetic clarinet tone of the Hammond organ, and also in that of the trombone, which also has a cylindrical bore. And speaking of bores, the author also tells us that flutes have tapering bores with the narrow end at the embouchure, but both Cecil Forsyth and Sir George Grove tell us that flutes are generally cylindrical, but that when a tapering bore is resorted to the broad end is always at the embouchure. One does not like to question Jean's statements, but one does hope that this book will receive the attention it deserves and that the author will be encouraged to prepare a new edition in which some of these difficulties will be elucidated.

OBJECTIVE ANALYSIS OF MUSICAL PERFORMANCE. *University of Iowa Studies. Studies in the Psychology of Music, Volume IV.*

Edited by Carl E. Seashore. University Press, Iowa City. \$2.00 (paper); \$2.50 (bound). $9\frac{1}{4} \times 6$; 379; 1936.

The science to which has been given the somewhat awkward but readily comprehended term of musicology is, like Gaul,

divided into three parts. First, there is the physical science of acoustics, with which the names of Mersenne, Chladni, Tyndall, and Helmholtz are indelibly associated. Second, there is the science of musical composition that reminds us of Fuchs, Marpurg, Cherubini, and Berlioz. And last there is the science of aesthetics which we owe chiefly to Herbart, Marx, Gurney, and Hanslick.

Almost without exception these authorities wrote and thought as if they lived in water-tight bulkheads, and the healthy growth of musical criticism has been greatly hampered in consequence. For too long it has been under the domination of the shallower minds of such writers as Ambros and Perry. Even such a really great aesthetician as Bosanquet is hopelessly at sea when he discusses music, and Lessing wisely ignored this art altogether.

Within recent years the tide has turned. Seashore and his satellites at the University of Iowa have undertaken a psychological examination of musical compositions and their interpretation that seems likely to go far toward establishing a scientific foundation on which may be erected an intelligent aesthetic in the future. The present work is the fourth volume in a series devoted to this end. It is not simple reading but demands all the mental concentration that the reader can muster. But he who reads these essays will be amply repaid for his trouble, whether he be composer, performer, or critic.

Perhaps the most striking essay in the collection is one devoted to the analysis of violin playing relative to the Pythagorean, equal, and just systems of temperament. Almost without exception the critics have dogmatically stated that violinists who play unaccompanied instinctively play in just temperament, yet the outcome of this investigation is that in every instance the standard deviation in pitch was least when measured from the Pythagorean intonation, and greatest when measured from the justly tempered intervals. It would have been interesting had the investigator distinguished between essential and inessential dissonances, for it is conceivable that the deviations might have been different in the two cases. Such an investigation is badly needed.

There is one typographical error—one chapter heading refers to the first movement of one of the Beethoven piano sonatas, but an examination of the score shows that the last movement was intended. But details like this do not detract much from the value of the work, which is really deserving of a great deal of commendation.

Sleep Characteristics. *How They Vary and React to Changing Conditions in the Group and the Individual.*

By N. Kleitman, F. J. Mullin, N. R. Cooperman and S. Titelbaum. University of Chicago Press, Chicago. \$1.00. $8\frac{1}{2}$ x 6; vi + 87; 1937.

The subjects were selected from the laboratory personnel—students, instructors, employees, and a few outsiders. Data were collected on the amount of motility during sleep, number of hours slept, continuity of sleep, dreaming, spontaneity of awakening, etc. The effects of climatic conditions, food intake, and the subjective condition of the person at the time of retiring were all studied. Numerous tables have been included in the text. The material has been analyzed statistically, with special emphasis on variance analysis.

Educational, Psychological and Personality Tests of 1936. *Including a Bibliography and Book Review Digest of Measurement Books and Monographs of 1933–36.*

By Oscar K. Buros. School of Education, Rutgers University, New Brunswick, N. J. 75 cents. 9 x 6; 141; 1937 (paper).

This bulletin is a non-cumulated number including only tests published in 1936 and tests not included in earlier bibliographies, 1933–35. Practically all 1936 pencil-and-paper tests published as separates in the United States and the British Empire, and many non-pencil-and-paper tests are included. The contents include a bibliography of tests, a bibliography of

books with review excerpts, a publisher's directory and index, a periodical directory and index, and an index by titles and by authors.

Food-Tokens as Incentives for Learning by Chimpanzees. *Comparative Psychology Monographs, Volume 14, Number 5. Serial Number 71.*

By John T. Cowles. Johns Hopkins Press, Baltimore. $1.75. 10 x 5⅞; 96; 1937 (paper).

Because human activity is concerned neither directly nor entirely with the need for food, animal experiments involving immediate food reward may not be comparable. This experiment was to determine the efficacy of substitute rewards which lead to the acquisition of new habits. A number of position and visual discrimination patterns from simple to complex were used, and in each case the responses to direct food, food-token, and non-food-token rewards were compared. The results are summarized and discussed.

The Intelligence of Animals.

By G. C. Grindley. Methuen and Co., London. 2s. 6d. net. 6⅝ x 4¼; vii + 70: 1937.

A short review of the experimental work that has been done on conditioned reflexes, maze learning, and of various other experimental techniques that have been devised for testing an animal's ability to reason. It is made abundantly clear that very little is known about animal intelligence.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

The Advancing Front of Science.

By George W. Gray. Whittlesey House, McGraw-Hill Book Co., New York and London. $3.00. 9 x 6; xiii + 364; 1937.

Here is an attempt to cover the entire domain of modern scientific activity within the covers of a single volume. Anyone who wishes to read it intelligently had better give up at the outset all idea of doing anything else at all until he is finished, for it will require all of his attention. In the preface the author extends thanks to about fifty collaborators, to consult all of whom constituted no mean task in itself.

In the opening chapter we are informed that the three fundamental problems awaiting solution at the hands of science are (1) The relativistic formulation of the quantum theory, (2) The nature of the atomic nucleus, (3) The nature of life. Any one of these seems like a pretty big order, but the author has courageously tackled them all, not of course with the idea of finding the solutions, but to show what has already been accomplished, and what is now being done in this direction. There are nine finely printed pages of index.

At the beginning of the century much of the traditional science of the past suddenly collapsed. The measurements of the velocity of light by Fizeau, and by Michaelson and Morley initiated a revolution in physics, the discovery of radioactivity did the same in chemistry, as did the rediscovery of Mendel's work and the genetic experimentation that followed it in biology. The reader will find the latest phases of all of these discussed in this remarkable production.

Unfortunately, there are no illustrations. The reading matter is a little heavy and some good illustrations would have been very helpful enabling the reader to follow the writer's thought. But nevertheless it is a fine job of the highest type of popularization.

The Tyranny of Words.

By Stuart Chase. Harcourt, Brace and Co., New York. $2.50. 5½ x 8; xiv + 396; 1937.

The wholly laudable intent of this book is to make the general public more aware than it now is about the importance of semantics for the business of living. There appears to be little doubt that if people generally paid more attention than they do to the meanings of words they glibly use our communal troubles might be diminished. Almost certainly the mass of

humanity would be less easily hoodwinked by Pied Pipers in high places with seductive radio voices.

Particularly Chase has tried, with on the whole praiseworthy success, to translate the high points of Korzybski's important but difficult treatise *Science and Sanity* into less forbidding language than that in which its gifted but not exactly pellucid author originally crouched them. The result is a book that every graduate student in science, and particularly in biology, will greatly profit by reading. Oldsters might profit too, but realistically not much can be hoped for in that direction, so compelling is the protoplasmic hysteresis that increases with advancing age. But there is another reason apart from its main objective that will make this book entertaining and useful to biologists. It contains a great many shrewd, amusing, and sound observations about the behavior of the author's tomcat Hobie Baker. Indeed one is compelled to wonder whether Mr. Chase might not have better fulfilled the higher cosmic purpose if he had devoted his life to the serious scientific study of the behavior of the lower vertebrates instead of just being a writer about human social and economic oddities.

The book has a brief but well selected bibliography, and an excellent index.

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THE QUARTERLY REVIEW of BIOLOGY

THE CONCEPT OF PURPOSE IN BIOLOGY

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I. INTRODUCTION

THE propriety and utility of the concept of purpose in biology is such a perennial subject of discussion that some justification for adding to the literature on the matter seems required. This justification is partly the importance of the matter, and partly a belief that there are certain aspects of the problem which have not received adequate attention in these discussions. Briefly, these concern, firstly, the fact that a living organism is not a single agent, but a nexus (both spatial and temporal) of agents; and secondly, the distinction between short range purpose as applied to a single act of such an agent, and long range purpose as applied to the end to which a series of acts is directed.

The importance which a biologist ascribes to the problem of purpose must be influenced by his special interests. There are great tracts of biological research where the problem of purpose is as irrelevant as quantum physics for engineering. But there are other regions of biology where it is of the first importance. For instance, the biologist may aspire to understand the behavior of an amoeba or of a developing egg in the same sense (even though much less completely) as he understands the behavior of his fellow man or his dog. No amount of knowledge of the chemistry and physics of this behavior would give this kind of understanding. At most, such a knowledge would tell him that the chemical and physical processes underlie the macroscopic behavior by which he judges that the man or the dog is acting purposefully.

Again, one of the special interests of biology is to arrive at an understanding of the process by which the higher organisms have evolved from the lower, and since we have primary self-knowledge that one of the higher organisms can act purposefully, to deny purpose in the lower organisms must involve a radical breach of continuity somewhere between the lower organism and man. Hence an adequate theory of evolution cannot be formulated without forming a judgment as to whether or not purpose is characteristic of all living organisms.

Some scientists and philosophers regard the purposefulness of which we are intuitively aware in ourselves as having

no causal significance. To them it is simply the mental correlate for a physical process, to which alone causal efficacy is to be ascribed. This question will be discussed in Sections 6 and 8; in the meantime we will assume the causal efficacy of purpose where it occurs.

Purposive interpretation of the responses of organisms (including, for instance, the responses of embryonic or tissue cells) is open to the charge of anthropomorphism. But if we regard man as a part of nature, and not as a unique being observing nature from the outside, we cannot regard anthropomorphic explanations as wrong in principle, though they may be erroneous, or useless, in specific cases.

Two uses of anthropomorphic explanations are inadmissible. The first is when such an explanation appears to be false in fact. For example, if one were to say that the purpose of an egg is to develop into the adult, the statement would almost certainly be false, having regard to the complex nervous apparatus associated with long range purposes in man, and to the considerations developed in Section 4. The other inadmissible use of an anthropomorphic explanation is to use it as a substitute for physical or physiological research. If one states that the cells of the amphibian blastula respond purposively to the stimulus of the evocator (a statement which we will maintain to be true) this must not be used as a substitute for research into the nature of the evocator substance, the changes taking place at the surface of the receptor cell, the physical relation of this to the subsequent reactions of the cell, and so on. But this applies to human purposeful activities also. The recognition that a man's act is purposeful is not a substitute for examining the neural and other processes involved in the act. And conversely, a description of these processes should not be used as a substitute for the conclusion that the act was purposeful.

In this paper I am not setting out to *prove* that purpose is characteristic of vital acts in general. No proof seems conceivable in the sense that other types of explanation would be excluded. As Woodger says (1929, p. 485) "It is always possible to defend microscopic mechanism *in principle*, if any one wishes to do so, by making your mechanism complicated enough, and by 'postulating' enough submechanisms to meet all contingencies. It *cannot* then be refuted, but neither can it be verified." And other types of explanation besides mechanism are in the field, for instance, organismic or Gestalt theories, which we will discuss later. But in this paper, I am starting out from the premise that we have first hand knowledge of the purposefulness of our own conscious behavior, and that it is good scientific method to seek to interpret the acts of other living organisms in terms of the acts of the organism we know best, unless there is good reason to suppose that they are radically different.

We will start, therefore, with the assumption that it is reasonable to suppose that the acts of living organisms, or living constituents of organisms (for example, cells) are directed towards ends, and discuss how far this is consonant with observation; what are the ends to which the acts are directed; what are the agents which are acting thus; and finally we will come to grips with a theory of causation which includes purpose as a causally efficacious factor.

In this discussion I will use the word *purposeful* for an act directed towards an end or goal when that end is present in consciousness, and *purposive* for an act directed towards an end when we have to leave undecided the question whether the end is present in any consciousness or not.

If the argument of this paper is correct, responses of tissue cells to stimuli are purposive acts, though we may not even be aware that these are going on. But the man is not the agent concerned in the response of a tissue cell—the agents are the tissue cells themselves, and we have no adequate grounds either for ascribing, or refusing to ascribe, consciousness to tissue cells. A purposive act, in the sense we are using it, is always the act of an agent, and therefore, in considering whether an act is purposive or not, the first task is to identify the agent concerned. The word *agent* will be defined and characterized in Sections 5 and 7; at present we can use it in an obvious sense.

The philosophical aspect of the discussion which follows has been profoundly influenced by Whitehead's Philosophy of Organism (1929), as I understand it.

2. SOME GENERAL FEATURES OF PURPOSIVE ACTS

This section deals with the macroscopic rather than the microscopic feature of purposive acts (molar, as opposed to molecular, behavior, in Tolman's terminology).

There is no doubt that a very wide range of vital acts, from the development of an egg to complex instinctive behavior, bears at least a superficial resemblance to our own consciously purposeful acts. Before discussing this resemblance and what it implies there is, however, one point of difference to be disposed of.

Our own purposeful behavior is directed towards an end in view which is dependent upon past experiences. The goal is imaged in terms of memory of past acts and their results. In the case of the development of an egg, or a spider spinning its web for the first time, whether there is any image of the future action or not, there can be no image depending upon the memory of past experience. In ourselves, a disposition to act in a certain way in a certain situation seems to be overwhelmingly formed by past experience. But we have to accept the fact that such a disposition can also be formed by the processes of embryonic development. Consider this simple example. Some animals instinctively seek the light, others instinctively avoid it—that is to say, the constitution of the organism as developed from the egg is such that it has the disposition to act in this way. Now it is easy to take an animal which by nature avoids the light, and by giving it an electric shock whenever it runs out of the light into the shade, give it a disposition to avoid darkness and seek the light. The behavior of this animal is now the same as that of another species of animal which is by nature positively phototactic. Placed in the dark, it comes out into the light, and if forcibly kept in the dark will exhibit the same objective signs of fear and striving that it previously did when forcibly kept in the light. It seems that one must accept the fact that there is no essential difference between innate dispositions to specific action in specific situations which have been formed by the ordinary processes of morphogenesis, and those which have been acquired by the modification of innate dispositions by experience. The fact that apparently identical behavior dispositions can be produced by such different processes as morphogenesis and learning seems quite mysterious on current ideas regarding both these processes, and ought to furnish a clue to a deeper understanding of them. This, however, is not directly our present concern, and I will use the term *action disposition* for that state of an organism which results in its responding in a specific way to a stimulus, irrespective of whether this state is the result of morphogenetic

processes alone, or of these modified by relevant experience.

The objective grounds for ascribing purposiveness to vital acts are familiar, though not always acceptable, to everyone. Firstly, such acts typically have results useful to the performer, and this useful end is reached by a series of acts which cannot of themselves be held to be of any use except as stages in the process of reaching the final useful end. The various motions gone through by a bird building its nest are quite meaningless except in relation to the final result.

The mere fact that the activities of living organisms end in results useful to the organism does not itself establish that they are purposive. This utility is capable of explanation, in principle at any rate, on familiar Darwinian lines. Granted that organisms vary in regard to their innate modes of response to stimuli, and that these variations are inherited, then the principle of Natural Selection accounts for the evolution of different types of organisms characterized by activities tending to the survival of these organisms. But though Natural Selection may be invoked to explain how it comes about that the activities of organisms have useful results, the question whether these activities are purposive or not is more fundamental. In fact, what selection does in this respect is to preserve those organisms whose acts are purposed to useful ends, and to eliminate those whose acts are purposed to harmful ends. The utility of the acts of living organisms is therefore not in itself sufficient to establish their purposiveness: rather, it constitutes a reason for examining the acts more closely to discover whether there are other grounds for concluding that they are purposive. One of these other grounds is the familiar fact that the sequence of acts by which the goal is attained is not always the same. On different occasions the organism reaches the same end by different routes. (By the "same" end is meant a "functionally equivalent" end). Even in the most rigidly instinctive behavior the animal has always to fit the details of its action to the special situation. The completed nest, the spider's web, the act of mating, is attained by a train of acts different in detail on every occasion. In embryonic development this is also the case. The most conspicuous example of this is the development of a functional adult, or organ, after the experimental introduction of profound disturbance in the early stages of development. But this is only an extreme case. As every embryologist knows, the course of normal development varies in detail, as does the performance of an instinctive action.

In the sphere of behavior this characteristic is exhibited as *trial and error* when elements of complete novelty are present in the situation, and by *insight* when it is a question of combining into a unitary action existing potentialities of response to elements in the situation which are not normally combined.

Many attempts have been made to eliminate the concept of purpose from the explanation of the fact that different routes may be followed to achieve the same end result. Perhaps the favorite type of theory at present is one which accounts for the facts on the principle used in Gestalt psychology; namely, that certain physical systems tend to a state where the potential energy is at a minimum. It is easy to imagine, or construct, such physical systems which will reach the same end state by intervening stages which differ according to differences in the initial configuration of the system. Let us adapt for our purposes the example given by Petermann in his critical discussion of Gestalt theory. If we have five equal floating

magnetized needles with the North Pole of each uppermost, and suspended above them a fixed magnet with the South Pole downwards, the five floating magnets will finally come to rest in a regular pentagon around the fixed magnet as a centre, no matter how they were arranged to begin with. Variations in the initial arrangements will affect the routes by which they attain their positions in the pentagon, but will not affect the shape and size of the final figure.

3. THE PURPOSIVE AGENTS

In this discussion I am conceiving purpose as characterizing the acts of agents; and this brings us to a source of confusion in the use of the concept of purpose in biology.

It is common, for instance, to distinguish between a reflex and an instinctive act on the grounds that the former is mechanistic, or at least not purposive, while the instinctive act is purposive. This distinction, however, surely involves a confusion as to the agents which carry out the two actions. Let us consider the reflex act of blinking when a speck of dust falls on the cornea, and the instinctive action, culminating in mating, which is the expression of the sexual instinct. To say that the act of blinking is unpurposed, and the behavior ending in mating is purposed, is to imply a comparison between incomparables. The reflex act is composed of a series of cell responses to stimuli—that is to say, the responses of a number of agents—the receptor neurone ending on the cornea, the motor neurone, the muscle cells, and perhaps others. This series of acts is to be compared, not with the acts of the mating animal, but with the entire series of acts composing the life cycle—namely, mating, the fusion of the ovum and spermatozoon, the development of the embryo, and its growth into the adult. To the external observer the series of actions making up the blinking reflex has the useful result of protecting the cornea, and so preserving the power of vision; the series of actions making up the life cycle has the useful result of continuing the species. But there is no agent that we know of which is purposing the preservation of the eye, nor of the continuance of the species. Purpose has to be looked for in the living agents which carry out each individual act of each series. The man is not the agent concerned in the reflex blink, nor is the species the agent concerned in mating, though the interests of each are served by the actions.

In each case, the purpose of the agent—whether it be the cells of the reflex arc, or the animal acting under the sexual urge—is to fulfill its nature (a phrase I will try to make more precise later). The animal has a disposition, an instinct, to act in a certain way in a particular situation. Its goal is to achieve mating. It has no concern with the wider "purpose" of ensuring the continuance of the species.

Consider again the agents in the reflex action, for example, the receptor neurone ending in the cornea. This may also be considered as having a disposition, an instinct, to act in a certain way in the presence of a specific stimulus—in this case, the irritation of a speck of dust lying on the cornea. Here again, the cell is no more concerned with conserving the visual function of the eye than the mating animal was concerned with the continuance of the species. Its goal is a much more modest one. It is simply to fulfill its nature by initiating a nerve impulse. Naturally none of the objective signs of purposiveness are to be observed in this act, for it is inaccessible to the necessary kind of observation. Moreover, even if it were accessible, it is doubtful if these signs would be forthcoming. For the extreme

simplicity of the act gives little scope for variation in the method of accomplishing it, or necessity for indirect methods of achieving the goal. Consequently, whether or not we are to attribute purpose to the neurone, in the same sense as we ascribe it to the mating animal, will depend upon indirect considerations which it is the object of this paper to develop.

4. HORMIC AND BIOLOGICAL GOALS

In order to distinguish between the vague, and surely unjustifiable, use of the word purpose in such a phrase "the purpose of the sexual instinct is the continuance of the species," and the precise and justifiable statement "the purpose of the animal under the sexual urge is to mate", we may perhaps describe mating as the *hormic goal* of sexual activity; there is an agent striving to that end. The continuance of the species, on the other hand, may be called the *biological goal* of sexual activity, because although sexual activity serves that end, we can identify no agent striving towards that end. There is no agent that achieves satisfaction by attaining the biological goal—or, at least, no agent that science at present knows anything about.

The present discussion is concerned only indirectly with biological goals. We are not concerned with how it comes about that the acts of an agent, or of a number of different agents collectively, form a series which has results of value to some organism other than the agents concerned. One may remark, however, that our present knowledge seems to leave us the alternative of an agent, unknown to science, to which the biological goal is a hormic goal; or Natural Selection of undirected variation of hormic goals which has brought about the survival of organisms whose hormic goals had these remote consequences.

Hormic goals themselves require a further classification into *immediate* and *terminal*. Consider the classical case of a wasp such as *Ammophila* which excavates a nest, then goes in search of a caterpillar. Having found one, it paralyzes it with its sting, and takes it to the nest. It then goes in search of another, and eventually, after the nest is sufficiently stocked, lays an egg in it and seals up the nest. It seems unnecessary to suppose that the hormic goal to which the first acts of this series are directed includes the entire series of acts ending in the stocked and sealed nest. It is enough to suppose that the hormic goal is always a little ahead of the present action. The hormic goal at the moment (stinging the caterpillar, putting it in the nest, and so forth) is the immediate hormic goal. The completed, sealed nest is the terminal hormic goal.

To assume that the achievement of, or approach to, one immediate hormic goal arouses another which carries the action further along towards the terminal goal which was not the hormic goal at the start of the action, is not to evade a difficulty. For this is a constant feature of our own experience. In many of our own consciously purposeful actions we have no clear idea at the outset what the final result of the whole business is to be. If we start to follow up a train of thought, we may arrive at a conclusion which is correct, but was quite unforeseen at the beginning, and which indeed may be very surprising to us. If we are unwilling to ascribe to the wasp at the beginning of its action, the purpose of carrying it all through to the completion of the nest, there is nothing more nor less mysterious in the action eventuating in the completed nest, than in our train of thought eventuating in an unforeseen conclusion. In both cases we have to accept the fact that the organization of the organism is such that

the achievement of one hormic goal evokes another one appropriate to the furtherance of the action.

The development of the egg has long been regarded by biologists as a test case for the application of physical as opposed to teleological theories of vital action. But the case against teleology has been exaggerated by confusion of hormic and biological goals. By a false analogy, teleological causation has been held to involve the hypothesis that the egg starts its development with the purpose of becoming an adult, and of "knowing how" to achieve this end. But the adult cannot be the hormic goal of the egg, if only because a considerable part of its development is carried out by relatively independent subagents (self-differentiating parts). And it seems certain that the hormic goals of these agents must develop with progressive action as we have described above. We will return to the problem of morphogenesis in a later section.

5. THE NATURE OF THE UNITY OF THE ORGANISM

The conception of the organism as an organization of agents raises the question of the nature of the unity of the organism. Although the question of teleogical causation can be considered apart from this, it is quite fundamental for any understanding of purposive behavior of the type which we actually find in living organisms.

At present, the tendency is to emphasize the unity of the organism as opposed to its multiplicity. And, of course, this unity is a very real and important feature, but should not be allowed to obscure the fact that this unity is a unity of agents which for many purposes must also be considered as individuals.

As discussed more fully later, embryonic development is characterized by the subdivision of the organism into parts, whose relation to the organism as a whole seems to be mainly that of responding to stimuli supplied by the morphogenetic fields, either directly or through the agency of chemical substances localized by these fields. But once these parts, or agents, have responded to these stimuli, they go their own way, often themselves subdividing into sub-agents which respond to the morphogenetic fields of the new parts. The essential independence of these parts is illustrated by the well-known transplantation experiments, where for instance, an amphibian limb-bud transplanted to a foreign site, such as the head, continues to develop as a limb. The relation of self-differentiating parts to the whole is little more than that of parasite to host.

Comparatively late in the development, these relatively independent parts, or agents, become connected up into a new unity by the development of the nervous system. But this new unity does not abolish the individual agents. It is a unity achieved, partly by the formation of a new connection between these agents, partly by the development of a dominant *Central Agent* which supplies stimuli to the sub-agents (effectors) in return for stimuli which other sub-agents (receptors) provide for it. The nervous system is, of course, not the only unifying factor in the adult animal. The hormone system must be reckoned as one of them. The unitary factor found in the developing embryo in the form of morphogenetic fields is also clearly present in the adult; the brain fields of Gestalt psychology, moreover, seem to be of comparable nature (Section 6).

Anatomically, cells are the most striking examples of the sub-agents which are thus mutually related to constitute the organism as a whole. But cells are not

the only, or perhaps even the most characteristic, agents. An agent is any region of activity which acts causally as a unit. This definition logically includes physical objects, an electron for example—and indeed in Whitehead's system the distinction between the living and the non-living is one of degree, not of kind. But now we are considering only living agents—that is to say, regions of activity which exhibit, in their degree, the features by which we commonly judge that organisms as a whole are alive. One obvious criterion is that when isolated from the rest of the organism, they continue for a time at least to manifest the phenomena of life; indeed their life seems to be dependent on their connection with the organism as a whole only in respect to extraneous factors such as supply of nourishment, and so on (as demonstrated by culture of parts of organisms *in vitro*). It is true that this criterion is not always available, but as a rule, it is not difficult to distinguish between parts of an organism which are alive, and parts lower down in the hierarchy of organization, such as atoms, which considered apart from the rest of the organism are not alive.

Cells, then, are only particularly conspicuous examples of agents in this sense. And there are indications that a cell may be a nexus of agents dominated by its own central, coordinating agent. This seems to be the case in most Protozoa, where cell parts such as cilia, and contractile vacuoles, have the same sort of relation to the cell as a whole as the cells of multicellular animals have to the organism as a whole.

On the other hand, we have to recognize agents involving larger regions than cells. Such are the regions carrying the morphogenetic fields, and also what I have called the Central Agent, the agent primarily concerned with the behavior of the organism as a whole. In the higher animals this is situated in the brain, in the sense that this is the place where it receives influences emanating from other agents, mainly through the nervous system, and from which it, in its turn, influences other agents. There is no need to suppose that its limits are closely bound up with any anatomical structure, or even that they are constant. The locus of the Central Agent is the locus of a nexus of processes which is part of a larger spatial and temporal nexus of processes acting and reacting upon each other.

The Central Agent is probably identical with the Ego of Gestalt psychology (Koffka, 1935). This Ego is a field process forming part of a larger field (its behavioral environment), the whole constituting the psycho-physical field. Any part of this field which forms a segregated whole within this larger field is a Gestalt, or agent in the terminology employed in this paper. In Whitehead's terminology, the Central Agent is a nexus of *presiding occasions*.

6. FURTHER CONSIDERATION OF THE NATURE OF PURPOSIVE ACTION

Firstly, what is purposed? Is it, for instance, the action itself, or is it the goal of the action? This would be a false formulation of the problem if by goal we meant the situation produced by the action. The goal of an animal under the sexual urge is not to bring about a state of affairs where mating has taken place, but to mate. The most primitive type of purposive behavior is probably simply to change a present state of discomfort or dissatisfaction into one of well-being or satisfaction. So if we are to draw any distinction between the action and the goal of the action, it must be between the action and the satisfaction which will

accrue from the action. But it is impossible to separate these two—one cannot separate the satisfaction from the means of obtaining the satisfaction. The possible satisfactions of an organism are specific to it, and imply the means of attaining them.

How different organisms came to have these different satisfactions is not part of our present problem—that is a problem of evolution. What is basic to all living organisms, and prior to the evolution of differences between them, is their striving to achieve satisfaction. This applies to the "organism as a whole," whether dominated by a Central Agent or not, and also to its various sub-agents—though the possible satisfactions are very different in the different agents.

Many biologists and psychologists would object to the statement that the purpose of the organism is to achieve satisfaction, and would say that the statement ought to run: It is a property of the organism to act in a certain way in a certain situation, in the same sense as we say that it is the property of oxygen and hydrogen under certain circumstances to combine into water, and that what I have called satisfaction is simply the completion of the process, the arrival at a new stage of equilibrium. The most sustained attempt to establish this view is probably to be found in the Gestalt system of psychology. For this reason, I must discuss the fundamentals of this system very briefly—especially as Köhler has extended Gestalt conceptions to the problem of the regulation of embryonic processes.

The Gestalt conception was originally founded upon consideration of visual perception. It was found that these phenomena displayed characteristics similar to those found in certain physical systems, especially those whose changes are describable on the principle that their potential energy tends to a minimum, as in the example of the floating magnets mentioned above. (The example chosen by Koffka is the tendency of a soap-bubble to assume the spherical shape.) This similarity is considered to be more than an analogy. The field properties of phenomena as experienced are believed to be due to the fact that these phenomena are the correlates of actual physical fields in the brain. Hence the total brain field is called the psycho-physical field. Such a field, or organised part of it, is a Gestalt.

Applied at first to visual perception, it has been developed, especially by Köhler and Koffka, into a complete explanatory system of mental processes and behavior. The Ego for instance, corresponding roughly to my Central Agent, is a special sub-system of the psycho-physical field, to which the rest of the field acts as environment. In his latest book (1935) Koffka says of the psycho-physical field "It is a system of stresses and strains which determines real behavior."

It is fundamental to Gestalt theory that the causal efficacy of the psycho-physical field is to be attributed solely to its physical properties. Although these fields may be the correlates of consciousness, the conscious aspect of the process does not enter into the causal explanations advanced by this theory. (Koffka, p. 65.)

Consciousness of purpose is simply awareness of stresses and strains within the psycho-physical field which are tending to change the total field in a direction towards greater equilibrium. This may be achieved in the case of human purposes by finding the correct idea which will solve an intellectual problem, or by appropriate bodily action where overt behavior is concerned. But the changes in the psycho-physical field which are the

correlates of the correct idea, or which result in the stimulation of the appropriate motor nerves, must be those changes which, under the conditions existing at the moment, will result in a minimum of potential energy in the system as a whole.

Gestalt psychologists, however, find it necessary to have recourse to the principle of *isomorphism* to account for the fact that physical processes in the brain result in behavior which deals successfully with real objects in the external world. According to this principle, there is a real correspondence between phenomena as consciously experienced and the physical processes in the psycho-physical field with which they are correlated; there is also a real correspondence between the processes in the psycho-physical field and external things.

The principle of isomorphism is also used by Gestalt psychologists to meet one of the old difficulties raised against the interpretation of thinking as a mere expression of physical processes in the brain. If the whole of the causal factors in thinking are physical processes in the brain, how is logical thinking possible, and how does it give us true information about the external world? According to Koffka, the intrinsic necessity of a logical conclusion corresponds to the intrinsic necessity of the dynamics of the physical field in the brain.

It is clear, of course, that since thought processes do issue in physical changes in the motor nerves, there must be a physical factor concerned. And indeed there can be no doubt that the general principles of Gestalt psychology are of great value to biologists. But surely the necessity of introducing the principle of isomorphism justifies us in continuing to use the concept of purpose without translating it into physical terms. For if there is a real correspondence between mental and physical processes, it seems just as permissible to give a causal explanation in terms of mental processes as of physical ones.

Let us return now to the processes actually involved in a purposive action, assuming that the end is a causal factor in the process. As we have seen, a purposive action implies both the striving after a specific satisfaction, and the means by which this may be attained. But it is cardinal to the whole idea of purpose that the means are only given in a general way, and have to be adapted to the particular circumstances.

What are the minimum assumptions we must make as to the underlying processes which make possible the adaptation of means to ends? One assumption is that every adaptive act—therefore probably every vital act—is preceded by a process which in the first instance I will call *imagined action*. In the final section we will find a more general formula for this process, which, in the form we are considering at the moment, is a highly developed form of a feature of all vital acts.

The term *imagined action* is adapted from the imagined movements (eingebildete Bewegungen) of Palágyi, who makes it one of the corner-stones of his Naturphilosophie. I use the term imagined action, rather than imagined movement, for various reasons, chiefly because I am including in the term the imagined satisfaction accruing from the movement, for as I have maintained, it is impossible to separate the satisfaction from the means of obtaining the satisfaction. Possibly a better term would be *imagined experience*.

Palágyi is emphatic that imagined movement is not the thought of the movement: it is rather the feeling of the movement. And this I suppose we may accept for our imagined action—it is

something prior either to the action itself or to the thought of the action.

According to Palágyi, every movement (action) is preceded by imagined movement (imagined action). He is referring to the higher organisms where the action is carried out by effectors stimulated by nervous impulse. Here the imagined action corresponds to neurological processes. But, granted that the acts of any organism are directed towards an end, the concept of imagined action seems logically necessary; for the end, even if it is to be achieved by a single muscular or protoplasmic movement, is in the future, and the physical cause of the action must be in the present.

The hormic goals of most purposive actions are not, however, to be achieved by a single act, such as a single muscular contraction, but by a series of acts. In such cases the organism (Central Agent) is experiencing imagined action which persists longer than the duration of a single act. This holds even in such a simple purposive action as movement directed towards an object, for slight deviations from the direct path have constantly to be corrected by subsequent movements. Each individual act is determined by a persistent factor which is the general imagined action and a variable factor which is the perception of the situation at the moment as it has developed from previous acts.

Imagined action is closely connected with perception. Indeed many psychologists would certainly say that it *is* perception. Let us rather say that imagined action is the way in which an object or situation is perceived. It is characteristic of present day psychology to stress the conative aspect of perception. Thus we perceive an orange primarily as something to eat, an angry dog as something to avoid. In its extreme form this is the bodily adjustment theory of perception: according to this theory, perception *is* the precursory adjustment of the body to the appropriate action. Behaviorists consider that this bodily adjustment consists of actual minimal contractions, or at least tensions, of the muscles. It does not seem necessary, however, to go so far as that. One may look upon imagined action as a certain orientation of the processes of the Central Agent. More precision will be given to this phrase in Section 8.

7. MORPHOGENESIS

This section is primarily concerned with showing that embryonic development may be interpreted as of the nature of behavior, especially of instinctive behavior; and therefore, if the previous argument is correct, is purposive. Two main problems confront us here. Firstly, the formation of sub-agents, the nature of their mutual relations and of their responses to stimuli; and secondly, the differentiation and development of the purposes by which their actions—forming in the aggregate the process of morphogenesis—are influenced.

An essential feature of the early stages of embryonic development is the multiplication of agents. The most obvious of these are the cells, but as has already been emphasized, the locus of an agent is not necessarily bound up with any anatomical boundaries. It may be the region carrying a morphogenetic field, for instance, and this may be the whole or a part of the organism. The conception of the sundering of morphogenetic fields into sub-fields is a familiar outcome of the experimental study of morphogenesis—for example, the sundering of a limb-field out of the whole, and its subsequent sundering into smaller fields dominating the development of the parts of the limbs.

In consistence with our general conception of the organism, the carrier of a morphogenetic field is an agent in that it acts causally as a unit. But although it is a unit from this point of view, it is a multiplicity from other points of view. It may contain, or be composed of, subfields which are also agents in that they may act as units within the main field. In this respect a morphogenetic field is comparable with a molecule, which in many respects acts as a unit, though composed of parts which also, under certain circumstances, act as units.

The causal action of these agents has now to be considered. The work of Holtfreter, of Needham, Waddington and Needham, and of others, has shown that one action of the dorso-ventral gradient field of the amphibian egg is to concentrate a chemical substance, the evocator, at the ventral end of the gradient (the organizer). Gastrular ectoderm cells lying above the organizer, even when this is artificially shifted from its proper place, respond to the liberation of the evocator by forming medullary plate. This is a one-way action, comparable to the response of a dog to the smell of a rabbit. In evocation, the relation between the causal and responding agents is an external one, and does not involve the relation which the two agents have to each other as members of the nexus of agents which make up the organism as a whole. This latter relation expresses itself as the individuation fields of Waddington (1933).

In the graded distribution of passive substances such as the evocator, or pigments, or of the character of physiological processes as revealed by the graded susceptibility to poisons in the axial gradients of Child, a morphogenetic field acts as a unit; for these gradations bear no apparent relation to the boundaries of the cells or other parts which together form the region which is the carrier of the field. But the field region is also a nexus of mutually related sub-agents, the cells or other parts just referred to. In its relation to these sub-agents, the field as a whole seems to perform two functions. It provides graded stimuli for them to react to, in this respect functioning as an individuation field. It also influences their reactivity to stimuli emanating from within or without the field—their competence (Waddington) to respond to morphogenetic stimuli.

A similar situation prevails in the case of the Central Agent of the adult animal. Whether it will respond to a specific sensum depends upon its "field"—that is to say, on the nexus of processes forming the brain field at the moment; moreover, the specificity of the response, if it occurs, depends not only on the nature of the sensum, but also on the character of the field at the moment. This is exemplified, for example, in the different responses to the sight of his master by a dog when he has nothing to fear from him, and when he has committed some act which he fears will bring punishment; or of an animal to a sexual object according to whether it is under the influence of sex hormones or not.

The conception of a morphogenetic field as a stimulus field (Reizfeld) has been developed at great length by Gurwitsch (1922, 1930), who also institutes a detailed comparison between the relation of morphogenetic fields to embryonic cells on the one hand, and the relation of brain fields to neurones on the other.

The agents concerned in morphogenesis are, like all living agents, characterised by a specific "action disposition," which the whole of the foregoing discussion implies is a mental disposition. These action dispositions are the morphogenetic potencies of the experimental embryologist.

In an earlier part of this discussion, it was maintained that the distinction between instinctive behavior and reflex action disappears when the agents concerned are properly identified. We may therefore use the word instinct to cover all action dispositions which owe their specificity to factors other than relevant prior experience on the part of the agent concerned. Instinctive behavior is thus contrasted with learnt behavior, though all learnt behavior is a modification of instinctive behavior. There seem to be no grounds for assuming that any learning takes place in morphogenesis. The whole process of morphogenesis is to be regarded as the unfolding of a complex instinctive behavior, involving the distinction we have already drawn between hormic and biological goals, and between immediate and terminal hormic goals. The total instinctive action is not performed by one agent, but is divided up among a progressively increasing number of agents with increasing specialization of action disposition.

It is noteworthy that a comparable situation occurs in the social Hymenoptera and termites. The development from the same germ plasm of different castes with different instincts, is strictly comparable to the development from the same egg of the agents concerned in morphogenesis. In both cases, the agents coöperate to achieve a biological goal which we may confidently assume is not at any time the hormic goal of any one of them. The biological goals are the building up and maintenance of the community in the one case, and of the organism in the other.

Thus we are not drawing a speculative picture when we conceive of a living agent, such as an egg, or germ cell, subdividing into a number of relatively independent agents with specialised instincts which coöperate to produce a biological goal, for this indubitably occurs in nature. The problem is, not whether it is inherently probable or improbable, but whether this occurs in morphogenesis as it does elsewhere.

There is one apparent difference between the responses of the agents in morphogenesis, and those of the "organism as a whole" (the Central Agent). The responses of the former characteristically result in irreversible changes of structure, which has the twofold result of making them incapable of responding again to the same stimulus, and of influencing their competence to respond to stimuli to which they were indifferent before. But irreversible changes may also occur as the result of adult behavior. The act of *learning* changes the competence of the Central Agent to respond to certain stimuli—a change that may last the life time of the organism.

When a rat learns to avoid a lighted alley which is provided with an electric shocking device, the essential change that takes place in the rat is a change in the way it perceives the light. The association of the light and the pain sensa has altered the Central Agent, or its brain field, so that now the light is perceived differently from its mode of perception before the association was formed. Before the experience the Central Agent was indifferent to the light, after the experience it became competent to respond to it.

This seems comparable to the change which takes place in the cells of the amphibian embryo, which in the blastula stage are indifferent to the evocator, but in the gastrula have become competent to respond to it—with the qualification that although the result of the changes from the one condition to the other are comparable, the factors which have brought about that change exhibit one

extraordinary difference—for relevant experience is causally concerned in the one case (learning) but not in the other; this however we have already discussed.

An act of learning is therefore a morphogenetic act, and results in the formation of an action or mental disposition which, as we saw earlier, may be indistinguishable from a disposition produced by the ordinary processes of morphogenesis.

8. PHYSICAL AND TELEOLOGICAL CAUSATION

The principal theoretical difficulty in accepting teleological causation is its apparent incompatability with physical causation. If every event is caused by a preceding event, where can we intercalate teleology into the causal series? In this section we shall try to find assumptions as to the nature of events and their mutual causal relations which will find room both for physical and teleological causation. The discussion will follow closely the principles of Whitehead's system (1929), as I understand it, so far as this is relevant to the special matter under consideration. But although what follows owes to Whitehead everything of value that it may have, I cannot be sure that I have interpreted him correctly in every point, and the extreme brevity of the exposition does not allow full justice to be done to him. Moreover, I have made my own applications of his ideas to the special purposes of this discussion.

The word *event*, as used in this discussion, means a *nexus* of elementary processes so inter-related as to form a whole when considered in relation to its causal effects on other events, including in these the elementary processes themselves which constitute the nexus. A nexus is a pattern, spatial, temporal or both, which is constituted not merely by the spatial or temporal relation of the parts, but by their mutual causal relations. Thus a molecule is a nexus of atoms, and an atom is a nexus of electrons and protons. In every case the elements of the nexus are considered in their dynamic, not in their substantial, character—as *processes* in Whitehead's terminology. Thus an atomic event is that nexus of electronic and protonic processes which endure for the smallest lapse of time necessary to constitute the system in its causal character as an atom. What we call *an atom* is the temporal nexus constituted by a succession of atomic events. Whitehead calls such a nexus, which maintains its character through a series of events, an *enduring object*. The limiting type of event is a single elementary process. Whitehead calls these *actual entities*, or *actual occasions*. As for the purposes of this discussion it is not generally necessary to distinguish between elementary events, and events which consist of a nexus of elementary events acting as a causal unit on other events, I will use the word "event" without always attempting to make this distinction. What I have previously called agents are enduring objects in the above sense.

All elementary physical events last at the most for a very small fraction of a second, but living organisms are characterized by events of longer duration. In man, for instance, the Central Agent is an enduring object, the events constituting which may last for the time interval known to psychologists as the specious present—up to say, $\frac{1}{10}$ to $\frac{1}{2}$ second in duration, or even longer. A single event in the Central Agent is therefore an event with a time duration of this order. It is necessary to bear in mind the conception of an enduring object, such as a molecule or the mind of a man, as a temporal

nexus of events with a certain continuity of character transmitted from one event to another.

Let us now consider physical and teleological causation generally, and specially in relation to the Central Agent.

As Whitehead points out, there are two ways in which we are related both to past and future. We may think about them, in the sense that we can re-construct in imagination the battle of Waterloo, or can speculate on the form our political system will take a hundred years hence. But the past and future are not related to us in this way only. We can feel the causal efficacy of our own past and future. This must first be considered in relation to our immediate past and immediate future—that is to say, the event, or nexus of events, immediately preceding and succeeding the event of the Central Agent occupying the specious present.

If we are making a sustained effort—say, following out a train of thought—we are aware that the character of our present thought (its *subjective form* in Whitehead's terminology) is directly determined by the preceding thought. We are also aware that our present thought is not a terminus, but is directed towards continuing the previous line of thought into the future. Our present thought (more accurately, our present thinking) is an event, growing out of the previous event and leading to the next one.

It is necessary to use other terms than memory and purpose to describe this causal relation of the present act to its immediate past and future, and we can employ Whitehead's terms *prehension* and *subjective aim*. An event of the Central Agent prehends its immediate past, and has subjective aim towards its immediate future.

Let us consider prehension first. To understand this, it is necessary to remember that the Central Agent is not an isolated temporal nexus of events, but forms a part of a larger spatio-temporal nexus of processes which constitutes the brain field, or psycho-physical field of Gestalt psychology. The Central Agent is one temporal series of such processes, possessing a specially intimate coherence. But it is also in intimate relation with the surrounding processes in the brain, and through them, with all the processes in the body. Let us call the totality of processes contiguous to the Central Agent at any moment, its field (though here there is possibly a departure from Whitehead's use of this term).

Prehension is the initial phase of the reaction of an event to its field of contiguous events. Every act of the Central Agent starts therefore by prehending its field of events, an important one of which is the immediately preceding event in the Central Agent itself. But the event has a certain duration; that is to say, certain internal processes have to take place in it before it is in a position to give rise to the next event, and subjective aim concerns this internal process.

It is in this conception of what happens within an event itself that Whitehead departs from ordinary views of the nature of physical causation, which concerns the effect of one event upon another. In any causal series, each event initiates the next; this is physical causation. But each event occupies a certain duration, even if it is only the period of a light wave. What takes place within this duration is outside the physical causal series. Its physical causal nature is that of a process occupying a characteristic temporal and spatial extent. Ultimately, any causal series must be analyzable into a series of events, each the effect of a preceding event, and the cause of a succeeding event, with an interval between the effect and the cause

which is the duration of the event itself. It is in this interval, that is to say, within the event itself, that teleological causation is to be looked for.

According to Whitehead, every event (even such an elementary physical event as an electronic vibration) arises as the effect of a previous event, or nexus of events (its cause), and has subjective aim towards its own completion (*satisfaction*) which will make it a physical cause of the next event. This position can be supported partly by a consideration of our own elementary mental acts, and partly from the physical conception that a chain of physical causation, in its last analysis, is not continuous, but spatio-temporally quantised into finite units. Subjective aim fills the causal gap between the initiation of one event by a previous one, and its own initiation of the next event. As the term implies, it is teleological causation; this is not only because it is directed towards the completion of the present event, but because this completion, or satisfaction, means acting as physical cause of other events. So subjective aim is aim at acting as the cause of other events, (or, following Whitehead's system more closely, aim at determining the character of other events).

But it would seem unnecessary to invoke subjective aim if this was all that was involved. It would be enough to define an event as a process arising as an effect, and becoming itself a cause after a certain spatio-temporal interval, or in other words, that a "causal unit" is not instantaneous, but occupies a small region of space-time (Bertrand Russell). And there seems no evidence that anything more is required for a description of physical processes, or of any process where there is no adaptation to ends. But where adaptation to ends takes place, something must happen within the event itself which will determine that the next event of which it will be a physical cause will be an event appropriate to that end. And this teleological factor is subjective aim. Whitehead, indeed, believes that subjective aim acts in this way even in physical events, but we do not need to follow him here, nor discuss his reasons for this conclusion.

Let us return now to the consideration of purpose in the usual sense of this word. Long range purpose, like memory, depends upon a certain persistence of character of the nexus of processes forming the field of a series of events or acts of the Central Agent. Such purposes have the character of reference to the future; but this is no more nor less mysterious than the fact that memories have reference to the past. And mysterious or not, both are facts. But for science, the problem of purpose is not its reference to the future, but the causal efficacy of this reference, and this is to be found in subjective aim, which concerns the self-completion of each single elementary event which will make it a cause of other events. In conscious behavior the total field of processes which is prehended by the present act of the Central Agent includes memories, sensations and so forth, which together form the action disposition of the organism at the moment, and sets the character of the subjective aim. Subjective aim concerns only a single event of the Central Agent, and long range purposive action is due to a persistent character of the field which is prehended by a series of events constituting a stretch of the enduring object which is the Central Agent. In the higher organisms, this persistent character may be a mental image of the goal. But a consciously conceived purpose, or end in view, is no more necessarily implied in teleological causation than conscious memory is implied in

every case where present action is influenced by past experience.

We have now to examine how teleological causation, or subjective aim, operates. As a preliminary, we may note that mere urge or drive, as such, over a series of events, or within a single event in the form of subjective aim, would not imply teleological causation. At least, it would be useless to invoke teleological causation merely for this. Science is only concerned with entities or functions that cause differences between phenomena, and unless subjective aim means the potentiality of making a difference in subsequent events, it is meaningless. At the very least, teleology, if it is to be causal, must be concerned with existing possibilities of success or failure to achieve an end. We must approach this problem also from consideration of our own purposeful behavior.

Suppose a man is suddenly confronted with a dangerous situation; that is to say, a visual sensum is prehended by the Central Agent in an event which has the subjective form of fear, with the corresponding subjective aim at escape. No teleology is concerned in determining the subjective form and the character of the subjective aim. These are the result of the configuration of the field of the Central Agent. But teleology is concerned with the mode of realization of the subjective aim. For the particular mode of escape—running away, standing still, various kinds of defensive action—depends upon the existing circumstances of the situation. There are various possibilities of action, of which one is to be selected as the one which appears the most suitable to the situation. This selection does not necessarily involve conscious deliberation. Of the various modes of escape, one is perceived as the most suitable.

But possibilities refer to the future, and selection has to operate in the present. What is the present happening that is selected? We will call this thing which is selected, *conceptual action* (partly equivalent to Whitehead's "conceptual feeling of an eternal object"), noting that, as in Whitehead's terminology, conceptual does not imply conscious, though it does imply mental. The forms of conceptual action available to an organism depend upon its bodily organization. For the events forming the field of the Central Agent include those which are the "feelings" of its various bodily parts or sub-agents, caused and maintained by processes in these parts transmitted to the Central Agent. And since conceptual action is to issue in overt action, that is to say, in changes in these bodily parts, the possible forms of conceptual action are dependent on these feelings. The fact that teleological causation has such great scope in living organisms is due to the fact that they are nexus of sub-agents, and therefore rich in forms of conceptual action.

The initial phase of an event in the Central Agent is therefore a prehension of its field of contiguous processes. This prehension gives rise to conceptual action—in fact, conceptual action is the form in which the field is prehended. But various forms of the general conceptual action of escape (in our example) are relevant to the situation, and the most appropriate of these has to be selected and the others rejected. Conceptual actions are possibilities of overt action—but only possibilities, because until one has been accepted by, or incorporated into, the event under consideration, the event does not become the physical cause of other events (in this case, the excitation of motor neurones). "Incorporation" means much the same as Whitehead's *concrescence*.

It must be admitted that the word

"event" seems to have a less precise meaning when used in this sense than when used for an elementary physical event such as a light wave, the spatio-temporal extent of which can be precisely defined. The feature which the two events have in common is that each must be considered as a unit in relation to its causal effect on certain other events. There is clearly a sense in which an event such as we have described in the Central Agent can be analyzed into parts; but considered from the point of view of its causal action in exciting motor neurones, the whole process is a unit. For the selection of the most appropriate form of conceptual action must be considered in relation *both* to the origin and termination of the event. Subjective aim sets the course of the event from its origin as a prehension of a field of events containing a particular visual sensum, to its completion as the physical cause exciting the motor neurones. It is for this reason that teleological causation is located within events, and physical causation at, so to speak, the junction of events. Whitehead, as already mentioned, finds it necessary to ascribe prehension, subjective aim, and satisfaction even to elementary physical events. But without going into this, the comparison with elementary physical events at least seems to show that even in that sphere we have to deal with physical causal units, or processes, which are not instantaneous but occupy a finite region of space-time. Thus no principle of physical causation is violated by locating teleological action between the effect of one link and the cause of the next link in the causal chain.

Perception is a conscious prehension, where the objects prehended are sense data, or sensa, provided by the sensory receptors. Conceptual action is simply the conative (rather than the cognitive) aspect of how the object is prehended or perceived. "Perception in its primary form is consciousness of the causal efficacy of the external world." (Whitehead, 1929, p. 169). I have already discussed this in connection with imagined action, which, when applied to a single event, is conceptual action. But it tends to confuse the issue to use the special term perception only for the prehension of sensa in the usual meaning of that word. For the processes giving rise to bodily pains and fatigues—in fact, any happenings in the "field" of the Central Agent—are perceived in this sense. This means that our own immediate past (the previous specious present) is also perceived. Whitehead (1933) calls this non-sensuous perception.

It is important to remember that teleology is not concerned in the *origination* of conceptual action. A man perceives an orange as something to be eaten, or a rabbit perceives a dog as something to be avoided, because it is their nature to do so—natures which have been determined by their whole past histories, ontogenetic and phylogenetic. If I am trying to solve an intellectual problem, ideas, more or less appropriate, arise in my mind. But these ideas—which are forms of conceptual action in which the conative aspect is at a minimum—arise spontaneously, so far as my will is concerned, according to the psychological principles of association. Teleology is involved in the admission or rejection of these ideas according to whether or not they are consistent with the subjective aim of the event in the Central Agent which has prehended them.

In a train of thought, the adaptation of means to end is carried out by selection of conceptual action which does not lead to overt action. That is to say, the completion of each event of the Central

Agent determines the character of the next event, but does not excite any motor neurones. In conscious thinking, the nexus of successive events of the Central Agent is perceived (in Whitehead's extended meaning of the term) as consciously imagined action, in which, however, the conative aspect may be reduced to a minimum. Typically, thinking is concerned with universals, because adaptive behavior is a fitting of a generalized type of behavior to a particular situation. To quote from Wolters ("On conceptual thinking," 1934):

> The problem of universals (confronts) us in animal behaviour as certainly as in human. A dog recognises any dog as a dog and any cat as a cat, as is clear from the appropriate direction of activities towards members of the two species. . . . The universal is found in the behaviour of the creature. . . . There is a readiness, or preparation, for activity, and it is general in the sense that it is available for a number of situations not completely identical. . . . Where we find this incompletely specialised readiness it is perhaps permissible to call the resulting behaviour "conceptual" in the case of animals as much as of human beings. The name is less important than the recognition that there is a mode of reaction common to men and animals which must be accepted as general in character, but which does not involve the recall to consciousness of any content.

We have discussed teleological causation mainly in relation to the events constituting the Central Agent; but the argument must be applied, in its degree, to all the living agents which constitute the nexus which is the organism as a whole, whether during embryonic development or as the adult: to all agents, that is to say, capable of adaptive action. The essential point is that teleological causation operates within the event itself, by selection of conceptual action. This is a phase of the incomplete event, between its origin as an effect of previous events and its own causal determination of the character of other events.

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THE ORIGIN OF THE FAUNA OF THE GREATER ANTILLES, WITH DISCUSSION OF DISPERSAL OF ANIMALS OVER WATER AND THROUGH THE AIR

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INTRODUCTION AND ACKNOWLEDGMENTS

THE purpose of this paper is to find how the fauna of the Greater Antilles, the four big islands of the West Indies, has been derived from the mainland, whether across the water or over land connections. This problem, like any other in biogeography, involves the recent history of the earth's surface. It is of interest to geologists as well as to biologists, and has been discussed many times, most recently by Schuchert (1935), who concludes that the fauna of the islands has come from Central America over land connections. However, a good deal still remains to be said. Some of what I shall say is elementary, but is necessary for a proper approach to the subject.

It must be remembered throughout that, except where otherwise stated, this paper deals exclusively with the Greater Antilles. The Lesser Antilles have had a different geological origin and have received their fauna largely from different sources, and to lump them in discussion with the Greater Antilles would introduce many complications. The origin of the fauna of the Greater Antilles is a sufficiently complex subject in itself, so much so that, in attempting to simplify it and to keep the length of this paper within bounds, I may occasionally be guilty of "reducing to baldness an argument that is entitled to hairsplitting."

During the writing of this paper I have become indebted especially to Dr. Thomas Barbour, who, although he does not agree entirely with my conclusions, has given me much information and repeated encouragement. Under Dr. Barbour's directorship, the Museum of Comparative Zoölogy is a perfect headquarters for work on the Antilles. I wish it were possible (but it is not) to acknowledge in detail the facts and ideas which have come my way from members of the staff during conversations on the back steps of the museum. Most of the meteorology in this paper has come from or been checked by Prof. C. F. Brooks of Harvard. Information received from specialists is acknowledged at various points in the text below.

THEORY AND MATHEMATICS OF DISPERSAL OF ORGANISMS ACROSS WATER GAPS

Biogeographers customarily refer to dispersal of terrestrial animals across water as "accidental dispersal" or "random dispersal." These terms, however, are bad ones. In this paper the noncommittal term "over-water dispersal" will be used in their place.

The first objection to the term "accidental dispersal", as applied to dispersal across water, is that many factors besides accident are involved. It is no accident that some organisms, because of their nature and behavior, cross water more often than others; and it is no accident that some islands, because of their nature and position, the direction of winds and currents, and the nature of neighboring land, receive more organisms than other islands do. There is, of course, no doubt

that the dispersal of individual land organisms over water is largely accidental (so far as that goes, accidents must occur also in dispersal over land), but in the course of time statistical or mathematical probability comes into play and determines what sorts of organisms cross water most often and what islands they most often reach. Even if there is only one chance in a million that any given individual of a species will cross a given water gap, out of many million individuals some may be sure to cross; and, other things being equal, they will be more likely to cross narrow gaps than wide ones and more likely to populate large and fertile islands than small or barren ones. The dispersal of species and genera may, therefore, be regulated by many factors even when dispersal of individuals is accidental. All this is self-evident.

The mathematical chances of dispersal across water gaps of varying width have not received much attention, and what little has been said about them has usually been wrong. For example, Schuchert (1935, p. 85) quotes and accepts a statement from Matthew to the effect that chances of dispersal of organisms across a barrier vary inversely as the square of the distance. Now the inverse square law, which applies to dispersal phenomena such as the radiation of light, really has nothing whatever to do with dispersal of organisms. Organic dispersal takes place in a plane along the earth's surface, and even if it were otherwise similar to dispersal of light, the simple inverse (as originally suggested by Matthew, 1915, p. 208), not the inverse square, of the distance would be involved. However, this pales to nothing beside a second objection. The inverse square law as applied to light allows for no loss during dispersal, but accidental dispersal of organisms implies the loss of a large proportion of individuals in transit. This is a process not just of a different order but of a different kind.

Take, as an example, two similar islands, one 100 and one 200 miles from a continent. If the chance is 1/1,000 (or 1 in 1,000) that an individual organism which for any reason starts out to sea in the right direction will live and reach the nearer island, the chance that an individual will live and reach the farther island is not ¼ as great (as would be true if chances varied with the inverse of the square of the distance) nor ½ (as if chances were in simple inverse to distance) but only 1/1,000 as great, or 1 in 1,000,000, for if only 1/1,000 individuals survive the first 100 miles, only 1/1,000 of the surviving 1/1,000 will be expected to survive the second 100 miles on the basis of chance alone. In general, if X represents the chance of an individual successfully crossing a gap of width m, the chance of an individual crossing a gap of similar nature but of width n should be $(X)^{\frac{n}{m}}$.

This formula is, of course, only an approximation. It represents the chances as determined by the death rate during dispersal, and would hold only if dispersal were in one direction, along a series of parallel lines. If dispersal were in all directions from a single point, chances would vary not only according to the formula $(X)^{\frac{n}{m}}$, but also inversely as the distance, and the formula would be $\frac{(X)^{\frac{n}{m}}}{\frac{n}{m}}$. Since, however, dispersal must usually be from numerous points along a coast line of appreciable length, the chance of gap n being crossed will usually be between the values given by these two formulae. If X' is taken to represent the chance of an individual crossing n, the full formula becomes

$$(X)^{\frac{n}{m}} \gtrless X' \gtrless \frac{(X)^{\frac{n}{m}}}{\frac{n}{m}}.$$

For the purposes of the present paper, however, $(X)^{\frac{n}{m}}$ is a sufficiently close approximation.

In the case of the two similar islands described above, $X = 1/1{,}000$; $m = 100$; and $n = 200$; and the chance of an individual crossing n and reaching the outer island is not over $(1/1{,}000)^2$, or 1 in 1,000,000. If there were a third similar island 300 miles at sea, the chance of an individual reaching it would be not over $(1/1{,}000)^3$, or 1 in 1,000,000,000. This is less than 1/100,000 as good a chance as would be figured from Matthew's inverse square law! If, on the other hand, there were 1/1,000 chance that an individual would reach the 300 mile island successfully, the chance of one reaching the 100 mile island would be not less than $(1/1{,}000)^{\frac{1}{3}}$, or 1/10.

I shall not apply this formula very closely to specific existing islands, for I think it is a mistake to apply mathematics too closely to biological phenomena of which we know as little as we do of dispersal of organisms. In nature, mathematical chances must be modified by many biological factors, as well as by differences in age and availability of different islands, direction of storms and currents, etc. The formula $(X)^{\frac{n}{m}}$ is not exact; it is merely a convenient mathematical approximation. All I wish to conclude from it is that the relative widths of water gaps are more important in dispersal of organisms than is usually realized, and that organisms which are dispersed across water from a continent to a series of islands are correspondingly likely to reach the nearest islands first and very unlikely to skip near and populate far islands directly. In other words, the chances are that organisms dispersed across water to islands will be distributed in an orderly fashion, the order depending to a great extent on the widths of different water gaps.

Biologists writing on the fauna of the West Indian islands have repeatedly stated or implied not only that dispersal across water is accidental, but that the distribution pattern which results is accidental and irregular, with related species scattered at random. These persons believe that organisms dispersed across water can be recognized by their erratic distribution as contrasted with the orderly distribution of organisms which have been dispersed over land connections. (See especially Barbour, 1910, p. 280, and other papers; and Schuchert, 1935, pp. 105–106, agreeing with Barbour's statements.) Actually, however, the formula $(X)^{\frac{n}{m}}$ shows that dispersal over water should result in a fairly orderly distribution of life, with related forms occurring in orderly series on adjacent islands, much as they would be expected to do if dispersal were over land connections. The formula $(X)^{\frac{n}{m}}$ shows, too, that the greater are the odds against dispersal of individuals and the more rare and accidental are the events which bring dispersal about, the more orderly should be the resultant distribution of species. If this is a paradox, it is a reasonable one. A strong-flying bird, for example, does have a better chance of getting from Cuba to Puerto Rico without becoming established on Hispaniola than a mammal has of being rafted around Hispaniola without landing there, and the bird is more likely to become irregularly distributed. It seems, then, that erratic distribution among islands, when it reflects organisms' characteristics at all, indicates *ease of dispersal of individuals;* not that dispersal has been by "accident"; and not that it has been by water rather than by land. Of course erratic distribution among islands must often be the result not of organisms' characteristics at all, but of differences in the age or suitability of the islands or in other inorganic factors.

All this leads to the conclusion that, even when the dispersal of individual organisms across water to islands is largely accidental, the fauna and flora which accumulates upon the islands is largely not accidental, either in composition or in distribution. There is no obvious reason why such a fauna and flora should be less orderly than a fauna and flora dispersed over land, for there are plenty of irregularities of distribution to be found on continents. Mere order in the distribution of life on a series of islands is not, therefore, necessarily evidence of former land connections. The real differences between a biota dispersed over water gaps and one dispersed over land connections are to be looked for, first, in the nature of the constituent organisms—whether or not they are mostly of sorts easily dispersed over water and found on oceanic islands—and, second, in the *details* (not the degree) of their orderliness—whether their distribution reflects the widths of different water gaps and the direction of winds and currents, or the direction of past land connections.

FACTORS CONCERNED IN OVER-WATER IMMIGRATION OF ANIMALS INTO THE GREATER ANTILLES

Widths of water gaps, present and past

To turn to the Greater Antilles, and to the question of the origin of their fauna, it is necessary to examine not so much the present geography of the region as its geological history, in order to get an idea of the conditions under which animals have reached the islands.

At present Yucatan, about 125 miles from Cuba, is the nearest continental land to the Greater Antilles. The mainland of Florida, however, is less than 140 miles from Cuba. The distance from South America to the Greater Antilles is relatively great, but is spanned by a chain of islands, the Lesser Antilles, with the widest water gap only about 90 miles. It is impossible to say what the relative chances of immigration of animals from different directions are under these conditions.

In the past, however, the distribution of land has been very different. The peninsula of Florida has existed only since the Miocene, and its southern tip only since the Pleistocene. Some of the Lesser Antilles are rather recent, too, dating probably from the Pliocene or Pleistocene (cf. Schuchert, 1935, p. 749), and the islands have probably never been connected together north of Barbados and St. Vincent (Schuchert, pp. 730–731), although there are differences of opinion about this. Yucatan, too, is recent; the outer part of the peninsula became land only in the Pleistocene. There are, however, banks off Honduras and Nicaragua which, although they are now under the ocean, are thought by geologists to have been elevated for long periods in the past. These banks may have formed a continuous land bridge from Central America to Jamaica and Cuba, or the bridge may have been incomplete, interrupted by relatively narrow water gaps.

In the past, then, chances of immigration of animals into the Greater Antilles from Central America have probably been much better than now; chances of immigration from other directions, poorer than now, although there is some disagreement about the Lesser Antilles. Whether the Honduras-Nicaraguan route was continuous dry land, or whether there was always a water gap, is a question which geology cannot answer and which Schuchert (pp. 109–110) has referred to biology.

It is true that Schuchert (1935, p. 109) says, "If the latter [geologists and stratigraphers] know anything at all, it is that the present Greater Antilles

are not oceanic or volcanic islands but consist of continental rocks and are now fragments of a once greater Central America", and it is true that Schuchert's palaeographic maps (following p. 767) show wide and persistent connections between the Greater Antilles and Central America; but, on the other hand, Schuchert also states perfectly definitely (pp. 109–110) that there is really no geological proof that the islands have ever been connected with the mainland at all. Schuchert's maps give some idea of the probable conformation of the Caribbean region in the past, but, in view of what has just been said, they must be used cautiously.

Direction, nature, and lifting power of winds

The winds of the Greater Antillean region, both trades and hurricanes, usually trend from the islands to the mainland, and for this reason it has usually been supposed that winds have had nothing to do with the origin of the islands' fauna. The matter cannot be dismissed so easily, however. The trades may never reverse themselves except at very high altitudes, but the records show that hurricanes sometimes do move from the mainland to the islands (Fig. 1). For example, in 1905 a hurricane originated with its center just off the east coast of Nicaragua, so close that the land must have felt the effects of the storm, and then crossed directly over Jamaica and over the eastern end of Cuba, and very close to Haiti. In 1906, a hurricane originated in the Gulf of Mexico and crossed the Yucatan Peninsula from west to east, and the storm center moved northeast across the Yucatan Channel and just north of

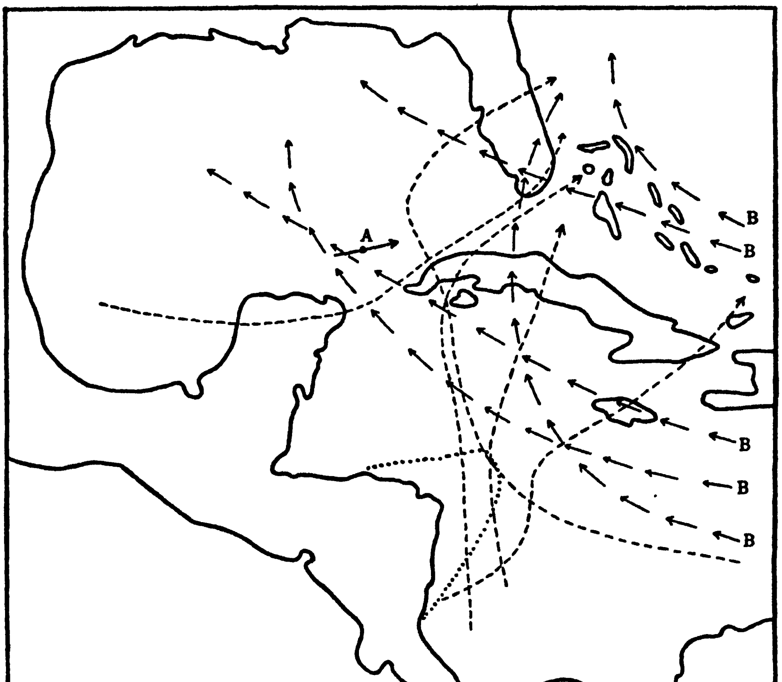

FIG. 1. PATHS OF THE CENTERS OF SOME HURRICANES (LINES OF DASHES) WHICH HAVE CROSSED FROM OVER OR NEAR CENTRAL AMERICA OR THE HONDURAS-NICARAGUAN BANKS TO THE GREATER ANTILLES (FROM SOURCES CITED IN TEXT)

Hurricanes like these are exceptional but not really rare. (Most West Indian hurricanes come in from the southeast; several typical paths are shown by series of arrows, BB.) Arrow at A shows approximate position and direction of progression of a hurricane center when wind of maximum velocity blows from Yucatan to Cuba; hurricanes followed approximately this path in 1895 and 1906.

western Cuba, which must have received the most severe part of the storm. Besides the few hurricanes which have traveled from the present boundaries of Central America to the Greater Antilles, others have crossed from above the submerged Honduras-Nicaraguan banks to the islands. If these banks were elevated, as geologists suppose they were, the number of storms crossing from the mainland to the islands would be increased. In any case, several hurricanes are known to have crossed from Central America to the Greater Antilles during the last 50 years, and many thousands have probably done so during the history of the islands. (Fassig, 1913; U. S. Weather Bureau map of hurricane paths from 1887 to 1929; *Cyclonic Storms*, U. S. Hydrographic Office, 1936; Tannehill, 1934; etc. may be consulted for further information and for maps of hurricane paths.)

It is necessary to know something about the structure of hurricanes to understand their possible importance in the dispersal of organisms. Hurricane winds are cyclonic or nearly circular, and, in the northern hemisphere, rotate about the storm center in a counter-clockwise direction (Fig. 2). The speed of progression of the storm center is rather slow, averaging only about 12 miles an hour in the tropics, but the cyclonic wind is often over 100 miles an hour, with gusts of 200 miles an hour or more. The ground velocity of the wind is greatest to the right of the path of the hurricane center, where the speed of progression of the storm is added to the rate of rotation; least at the left, where the ground velocity is rotational speed minus the speed of progression. The highest winds are usually not far from the storm center, but in very large hurricanes "destructive" winds may occur as far as 200 or 250 miles from the center, which means that they may blow across a gap of 100 miles or more with little curvature.

As the land now lies, almost any hurricane which passes through or near the Yucatan Channel may cause a wind to blow for a time from Yucatan to Cuba, no matter in what direction the storm center is moving. The wind will reach its greatest velocity from Yucatan to Cuba, however, during the passage of a storm toward the east along a path a little

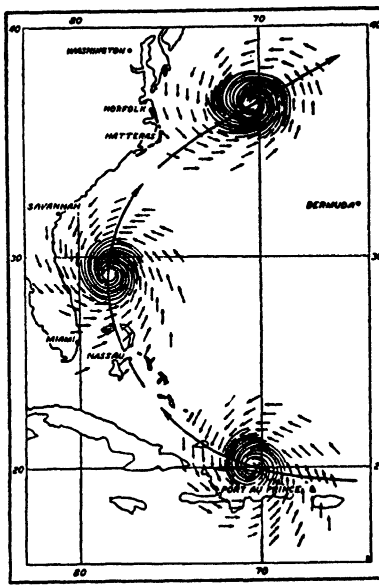

Fig. 2. Diagram of Track and Wind System of a West Indian Hurricane (from Tannehill, 1934)

north of the narrowest water gap (Fig. 1, A). Under the most favorable conditions the wind probably crosses the gap in not much over an hour, certainly in less than two hours. Storms followed the optimum path in 1895 and 1906 (the latter a different storm from the one mentioned above in the same year).

The lifting power of hurricane winds is, of course, as important as direction and velocity in dispersal of organisms. The center of a big hurricane is usually calm

for a diameter of from 7 to 20 miles. The inner cyclonic wind, nearest the calm center, angles upward, and the rate of ascent probably increases when the cloud zone is reached, for release of heat during condensation of rain forces an updraft of warm air which, it is believed, supplies the motive power of the whole hurricane. The central updraft of a hurricane is very

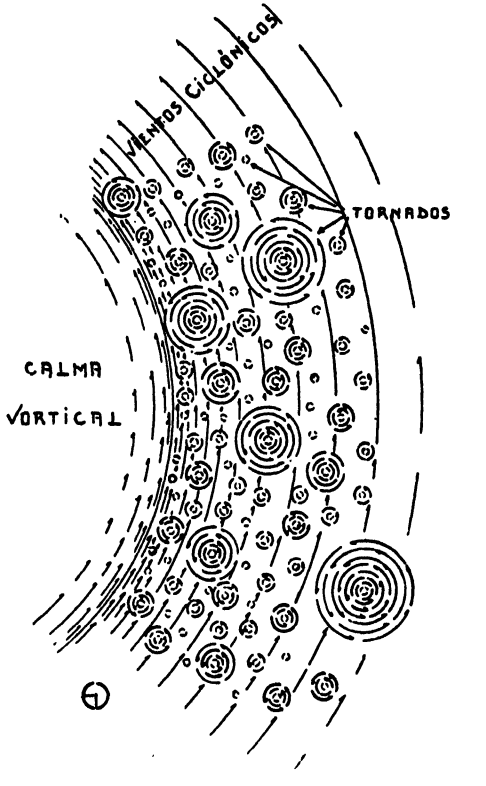

Fig. 3. Diagram to Show Probable Tornado-Like Nature of Gusts Carried by a Hurricane Wind (from Vázquez, 1936)

strong, and carries many thousand feet high. The main hurricane wind, however, except near the center, probably of itself has little lifting power, but the main wind carries with it gusts which seem to be of the nature of secondary whirls or tornadoes, and the secondary whirls sometimes have great lifting power. Violent tornadoes are known occasionally to have been carried by even the outer winds of hurricanes. For instance, in 1919 a tornado carried by a moderate hurricane wind moving at about 60 miles an hour, about 125 miles from the center of the main storm, cut a path 14 miles long and 100 to 600 feet wide southwest of Miami, Florida (Gray, 1919). Similar secondary whirls or tornadoes, varying in size and violence, are probably common near the centers of hurricanes. Vázquez (1936) gives a diagram (reproduced here, Fig. 3) of a hurricane wind literally packed with secondary tornadoes. It is interesting that one of the bits of evidence given by Vázquez to prove the tornado-like nature of hurricane gusts is the fall of live shrimp during hurricane squalls, which shows that the secondary whirls are sometimes strong enough to form waterspouts. One can get some idea of the sort of whirls or eddies which must occur in wind flowing at 100 miles an hour over irregular country, with a ground pressure of about a ton to the square foot, by watching the eddies in a stream of water flowing comparatively slowly over an irregular bed.

Not very much is known, really, of the details of structure of hurricanes even at ground level, and less at high altitudes, so that it is impossible to say how high secondary whirls carry debris or how long they hold it, or whether debris carried up by one whirl may be thrown into another or into the main updraft of the storm. But, from what is known, it is reasonable to suppose that hurricanes sometimes pick up fairly heavy objects and carry them rapidly and for long distances.

Men instinctively underestimate the lifting power of air. Many zoölogists to-day find it as difficult to believe that winds and rising air currents can lift and carry moderately heavy organisms as

persons once found it to believe that heavier-than-air machines could fly. There are several reasons why this should be so, why men should fail to appreciate the power of rising air currents and their effect on small organisms.

First, men live on the ground and are rarely exposed to rising air. They come to think of winds as chiefly horizontal forces, although meteorologists know that vertical air currents are really very common higher above the ground.

Second, men are large animals, and often do not realize the effect of air currents on smaller forms. Small animals are, of course, very much more at the mercy of winds than large ones are. The difference is due not directly to the difference in weight, but to the fact that small animals have a much higher ratio of surface to weight. The volume (and weight) of animals is a product of *three* dimensions and varies approximately as the cube of one dimension; the surface area of the body is a product of two dimensions, and varies approximately as a square. (Cf. the formula for the volume of a sphere, $\frac{1}{6}\pi d^3$, and that for the surface of a sphere, πd^2.) It is not necessary to demonstrate the mathematics in detail here, but it can be found that an animal 1 inch long has 65 or 70 times the surface for its weight as a similarly shaped animal the size of an average man. The ratio can be figured also from weight, and it can be found that an animal weighing 1 ounce (an adult house mouse weighs about 1 ounce) has about 14 times the surface for its weight as a similar animal the weight of an average man. These figures would have to be modified to allow for differences in shape as well as size, but it is not necessary to go into this complication here. Small animals certainly have many times more surface for their weight than large animals do, and air currents, which act upon the surface, have a proportionately greater effect upon small animals.

Finally, the pressure exerted upon a given object by wind varies not directly with the wind velocity but about as the *square* of the velocity. A wind of 100 miles an hour exerts about 16 times the pressure upon a given object as a wind of 25 miles an hour.

The formula for wind pressure upon flat surfaces, and its application to cylindrical objects, is briefly discussed by Humphreys (1929, p. 641). To calculate exactly the pressure of winds of different velocities upon objects of irregular shape is extremely difficult. It is hard enough to calculate the pressure upon simple spheres, as may be seen from Bilham's (1937) calculation of the rate of ascending air currents which support large hailstones. (It is interesting to note that, in theory, the largest spherical hailstone which can be supported by rising air currents other than whirlwinds is about 5 inches in diameter, although this maximum is hardly ever attained in nature.) However, the square of the velocity gives at least a rough idea of the relative power of a wind.

A wind of 100 miles an hour, then, has something like 224 (14 × 16) times the effect upon an animal weighing 1 ounce, and something like 1,040 (65 × 16) times the effect upon an animal 1 inch long, as a wind of 25 miles an hour has upon a man.

A man's experience, therefore, upon which his instinctive judgment is based, is that of a *large* animal, exposed for the most part to *moderate* winds, blowing *horizontally* near ground level. The effect of more severe winds upon small animals, even small fishes and frogs and mammals, may be hundreds of times greater in proportion to weight. And once an animal is carried above the ground by a whirlwind, it enters a zone where rising air currents are common. It is true that the figures cited above are only rough approximations, but they make it clear that a man cannot from his own experience estimate the effect of severe winds on small animals. Whether or not the latter

are carried by winds can be determined only by observation or experiment, and there are, of course, many actual observations which show that winds do carry small animals.

McAtee (1917) has given an excellent brief review of the transportation of various objects by wind and of the falling of such animals as earthworms, frogs, and small fishes with rain. Gudger (1921, 1929) has summarized about 70 more or less well authenticated falls of fishes in different parts of the world. There is no doubt that these phenomena occur and that the animals which fall have been carried up by whirlwinds or in waterspouts. Sometimes the animals come down alive, sometimes dead. Dr. Carl L. Hubbs has written me (letter of June 10, 1937) that, to test the possibility of dispersal of living fishes in waterspouts, he once dropped a number of small live fishes off the roof of a building, catching some in a pail of water and allowing others to strike on various wet and dry substrata. Most of the fishes lived.

The possibility of hurricane dispersal of some specific Greater Antillean animals will be discussed in more detail below. From what has already been said, however, about the direction, nature, and lifting power of hurricane winds, and about the ability of winds to carry small animals, it is clear that hurricanes must be taken into account in studying the origin of the islands' fauna.

Among the hurricanes which cross every few years from Central America to the Greater Antilles are some very violent ones. Such severe storms rarely cross from any other continental region directly to the islands. Only the relatively inefficient backdraft at the left of northward bound hurricanes blows from Florida to Cuba. The hurricanes which occasionally cross from the coast of Venezuela to the Greater Antilles usually follow a long, diagonal course. Storms rarely travel from South America up the Lesser Antillean chain, and the few which do so are usually not of hurricane violence. If hurricanes carry organisms at all, therefore, they probably have carried chiefly Central American forms to the Greater Antilles.

Besides hurricanes, small, local, cyclonic storms, like the tornadoes of the United States, sometimes occur in the Antilles. In Cuba, for instance, (according to data secured for me by Dr. S. C. Bruner) there are probably one or two such storms a year on the average. They are most common in the western part of the island, and usually occur in the warmer months, from June to October. Sometimes they form waterspouts. The Cubans call them, appropriately, *rabos de nubes* (cloud tails). Such local storms probably play some part in the dispersal of Antillean organisms, although they progress much less rapidly than tornadoes carried by hurricane winds, and are less likely to carry across wide water gaps. Even smaller and less violent whirlwinds may be of some importance in dispersal of island organisms. Perkins (1913, p. xlviii.) describes how, in the Hawaiian Islands, small whirlwinds sometimes carry columns of dust up more than 2,000 feet, and may carry small organisms into high air currents.

Ocean currents

Chances of dispersal of perishable organisms by ocean currents in the Antilles are probably not so good as by winds, although resistant seeds and animals which can stand long submergence in the sea must constantly be carried about. (See Guppy, 1917, for data on seed dispersal by ocean currents.) The course of the Gulf Stream seems not to favor the rapid transportation of organisms from the mainland to the islands. Local surface currents set up by hurricane winds, which sometimes blow very hard for a day or two in one place and direction, at one side of a nearly stationary storm, may

carry some organisms in floating debris, especially since hurricane rains are apt to flood rivers and wash out much drift material, but it must be remembered that the sea is very rough at such times.

Past differences in climate

It is likely that, in the past, meteorological conditions in the Antillean region have been very different from what they are now. It is probably idle to speculate much about this except for one thing. Before the Pleistocene, the climate of the whole world was much more uniform than it is now. The trade winds, which now sweep regularly from the Greater Antilles to the continent, owe their strength to the difference in temperature now existing between the poles and the equator. When the temperature differential was smaller, the trades were probably weaker and possibly more erratic, and the direction of progression of hurricanes, which depends partly on the direction of the prevailing winds, may have been more erratic too, so that chances of storm and drift dispersal in various directions may have been much better than now.

However this may be, the known facts suggest that the position of the islands, with the narrowest water gap on the Central American side in the past, and the direction and nature of storms have favored immigration of organisms into the Greater Antilles chiefly from Central America, even if there has never been a complete land connection.

Possible means of dispersal of Antillean animals across water, and answers to arguments against an over-water origin of the Antillean fauna

The most common argument against an over-water origin of the Greater Antillean fauna is that there are animals on the islands which *cannot* have crossed water barriers in any way. However, from what I have seen during collecting trips to Cuba, Jamaica, and Haiti, I think that this is incorrect, and that there is really no Antillean animal the ancestor of which cannot reasonably be supposed to have crossed water.

There are no animals on the Antilles too large to have been rafted across water gaps. It is probably true, as Barbour (1916) has said in criticism of Matthew (1915), that chances of raft dispersal are not good in the West Indies to-day, but in the past, before Pleistocene differentiation of polar and equatorial climates, the prevailing winds were probably lighter and the sea smoother, and chances of dispersal by natural rafts may have been better. Dispersal of delicate animals which cannot stand salt water may have been through the air. I have tried to show above that chances of aerial dispersal by hurricanes are very much greater than is usually realized. Animals which cannot be carried about free in the air might be carried in plant debris, and there is much debris in Central America and the Antilles capable of carrying animals. There are, for example, various palms which have huge leaves with sheath-like leaf bases.

According to Dr. L. H. Bailey (in letter of May 4, 1937) the West Indian royal palms (Roystoneas) have the most extensive leaf sheaths of any palms. The continental palms most like them are a few species of Euterpe in Guatemala, Nicaragua, and Costa Rica. Dr. Bailey points out that there are other plant structures, especially the floral spathes of some palms, which might carry organisms. Ordinary rolled dead leaves, too, especially large ones like those of the common Cecropia tree, might carry small organisms. However, I have chosen the palm leaves for special discussion because they are large and may carry relatively large animals; because palms of the right sort are native in both Central America and the Antilles; and because I know something of the fauna of fallen palm leaves.

The leaf bases of these palms are very large, six or eight feet long and two or three feet wide. When a leaf dies and falls to the ground, the base usually rolls up. The dead leaf is then about as near as nature can get to a kite four or five yards long with a large, tubular basket at one end. Sometimes the leaf base rolls into a smooth cylinder from which the contents might easily be shaken, but sometimes it rolls irregularly or becomes choked with trash. Such a leaf is fairly heavy, especially when water soaked, but it is nevertheless far lighter for its area than many objects which are known to have been carried by wind. McAtee (1917, pp. 217–218) gives some examples of the lifting power of whirlwinds. They have been known to carry even solid wood and metal for considerable distances.

The rolled boots of fallen palm leaves actually are favorite hiding places of many Antillean animals. The larger animals occupy the cavity at the center of the roll; smaller ones often wedge themselves between layers of rolled tissue. I have often collected in palm boots not only a variety of insects and ground mollusks and other invertebrates, but snakes and lizards and frogs. I have seen earthworms in palm boots over which damp leaves had drifted. *Peripatus* probably occurs in palm boots, too, although I have never actually found one there. Dr. A. M. Chickering writes (letter of Aug. 4, 1937) that he has collected *Peripatus* in fallen palm leaves on Barro Colorado Island, Panamá Canal Zone—not, indeed, in palm boots, but against the midribs of the leaves, between leaflets which had folded together. A single rolled palm boot may house a large number and variety of small animals such as these.

It seems to me that any ground-living animal which does not weigh more than a few pounds *might* be carried in the rolled boot of a dead palm leaf by a hurricane. Even small mammals might be carried, especially the young, in nests. The tough, rolled palm boot, jammed with nest material, would give a good deal of protection both in the air and in landing, and a nest might contain more than one young, which would increase the chance of a species becoming established in new territory. It is, however, doubtful if viviparous fishes could be carried in palm leaves, although the latter often fall boot-first into ponds and become choked with mud and water weeds.

Neither Dr. Barbour nor Dr. Hubbs thinks it likely that freshwater fishes have reached the Antilles through the air. Dr. Hubbs (letter of June 10, 1937) suggests that some or all of the few sorts of freshwater fishes which have reached Cuba could live foi some time in sea water, and that they may possibly have crossed through the sea, possibly at a time when heavy rains or a large river had somewhat sweetened the sea's surface.

Of course the best way we have of knowing whether particular sorts of animals really do cross ocean barriers is by whether or not they occur on known oceanic islands, and this test shows that birds and bats and insects and land snails do sometimes cross very wide water gaps. See Gulick (1932) for some of the biological peculiarities of oceanic islands, but remember that the islands with which Gulick deals are much more remote and much smaller than the Greater Antilles. Unfortunately no evidence of this sort is available for many kinds of Antillean animals, or rather the evidence is doubtful because of doubt as to the history of certain islands.

For example, it is supposed by most geologists and many biogeographers (cf. Schuchert, 1935, pp. 108, 729–730, 751, 752) that the Lesser Antilles, north of Barbados and St. Vincent, are true oceanic islands which have never been connected by land. If so,

their fauna, which comprises most of the classes of animals found on the Greater Antilles, including some terrestrial mammals and *Peripatus*, but probably not true fresh-water fishes, has reached the islands across water. Unfortunately, however, although there is no doubt that most of the Lesser Antilles are volcanic islands and not remnants of a continuous ridge, the islands stand upon a shelf which is nowhere deeper than 1,000 fathoms and for the most part less than 500, and the shelf may possibly at some time have been elevated so as to permit migration of animals.

There are, then, animals on the Greater Antilles of sorts which are not known on definitely oceanic islands. It cannot be proved that such animals are able to cross ocean barriers. Nevertheless, as I have tried to show above, they may have done so. Objections to over-water origin of the Greater Antillean fauna merely on the ground that some of the animals could not in any way have crossed water do not seem to be valid.

It may be asked why, if animals have reached the Greater Antilles over water in the past, they do not do so now. A few small animals, including even some reptiles and a frog, may have reached Cuba rather recently, for they belong to mainland genera not found on any of the islands except Cuba. However, among such groups as the terrestrial mammals it seems that no species has reached the Greater Antilles for a very long time, except for species introduced by man. This, though, does not bar out the possibility that mammals came over water in the past, when weather conditions may have been more favorable to rafting and when water gaps were probably narrower.

Suppose that the ancestors of the Greater Antillean mammals reached the islands across a water gap of 80 miles, about the distance between the Rosalind and Pedro Banks if the banks were elevated 100 fathoms. Then what would be the chances, relatively speaking, that mammals would cross the present 125 mile gap between Yucatan and Cuba? The formula to use is $(X)^{\frac{n}{m}}$, and $\frac{n}{m}$ = about $\frac{3}{2}$. If there were 1/100 chance that an individual mammal would cross the Rosalind-Pedro gap successfully, the same mammal would have less than 1/10 as good a chance to cross the present Yucatan Channel; if the chance of the first crossing were 1/10,000, that of the second would be less than 1/100 as good; etc. And if dispersal has been by hurricanes, there is an additional factor to be taken into account. Hurricane winds (Fig. 2) are nearly circular and are much more likely to blow directly across narrow gaps than wide ones. The width of a gap must be even more important in dispersal by hurricanes than the formula $(X)^{\frac{n}{m}}$ shows.

All this suggests that animals which may have been reasonably likely, in the course of a long period of time, to cross a gap of 80 miles from Central America to the Greater Antilles may have almost no chance at all to cross the present gap.

Another objection to over-water dispersal which has been raised repeatedly (Allen, 1911; Barbour, 1914, p. 236; Bates, 1935, p. 89) is based on the existence of local endemic subspecies and species (of bats, reptiles and amphibians, and butterflies) confined to different Antillean islands or to the islands as opposed to the mainland. It is argued that, if over-water dispersal occurred, it would cause intermixing of populations and would prevent the divergence of local forms, so that the very existence of the latter is evidence that there has been no dispersal across the water gaps. This reasoning, however, does not stand up against either mathematics or logic.

As an example, let us take a hypo-

thetical island inhabited by a species X, of which there are a million individuals reproduced in one generation a year, and let us suppose that on another island there is a related interfertile species Y. If one individual of Y reaches the first island each year and interbreeds with X, what will be the result? A simple calculation shows that, if only one part per million of Y germ plasm is introduced into X each year, and if there is no selection, it will require a considerable part of a million years for much effect to be produced. Selection of more than one X at the expense of one Y per million individuals, or any other equivalent evolutionary process, should more than overcome the effect of intermixture. Even if 100 individuals of Y interbreed with X each year, selection of more than one X over one Y per ten thousand individuals, or any other equivalent force tending toward divergence of X, should more than overcome the effect of intermixture. An evolutionary force equivalent to selection of one individual in ten thousand is surely not an unreasonably strong force. Yet arrival of even one individual a year on a suitable but unpopulated island is probably a much higher frequency than would be necessary to ensure establishment of a species. Local endemics might, therefore, originate even if dispersal were constantly occurring at a rate much higher than necessary to populate islands.

If this mathematical demonstration is not enough, two analogies ought to be convincing. First, endemics have frequently originated on small portions of continents, although migration must at some time have taken place over the land. How, then, can the presence of endemics on islands be taken to prove that migration was not at some time over water? Second, the Hawaiian Islands are probably true oceanic islands and the Hawaiian fauna has probably been derived from great distances across water, yet some of the animals, for instance many birds and beetles, have evolved local forms on different islands of the Hawaiian group which are separated by very narrow water gaps. If the Hawaiian animals really arrived across water (which, it must be admitted, a few persons doubt), the Antillean animals may also have done so, so far as the evidence of endemic forms goes. There is no doubt, of course, that water gaps are important barriers, but there is no evidence that they are completely impassable for any Antillean animals.

Nothing which has been said thus far has been intended to prove that animals really have reached the Greater Antilles over water, merely that they may have done so; that conditions have probably been much more favorable for over-water immigration than has usually been realized, and that the objections to over-water dispersal are not very serious. Whether the islands really have been populated over water or across land connections is, I think, a question which can be answered only by a study of the fauna itself, not by arguments about chances of dispersal of animals across water gaps. A study of certain characteristics of the Greater Antillean fauna will be attempted in the following pages.

CHARACTERISTICS OF GREATER ANTILLEAN FAUNA

It is pretty generally agreed that the fauna of the Greater Antilles has three chief characteristics. First, it is derived principally (but not wholly) from Central America, except that many of the birds have North American affinities (Bond, 1934), and that it is impossible to say with certainty from what direction the terrestrial mammals arrived. Second, the fauna is fairly homogeneous and very

orderly; the islands differ considerably among themselves, but they are faunistically much more like each other than like the mainland, and related animals are usually distributed in orderly series. Third, the fauna is in many ways strikingly depauperate; many groups of continental animals which are common even in Central America are absent on the islands. The multiplication of species and sometimes even of genera in some groups of animals on the islands, through radiative evolution and setting off of localized forms, should perhaps be mentioned as another characteristic of the fauna.

Fauna derived chiefly from Central America

Under this heading it is necessary only to repeat what has been said earlier in this paper, that the conformation of land in the past, with the narrowest water gap on the Central American side, and the direction of hurricanes have favored immigration of animals into the Greater Antilles chiefly from Central America even if there have never been complete land connections. The relationships of the fauna with Central America, therefore, do not necessarily indicate a Central American land bridge, nor can geologists say for sure whether or not such a bridge has ever existed.

Homogeneity and orderliness of fauna

The homogeneity and orderliness of the Greater Antillean fauna have frequently been invoked as evidences of past land connections, at least among the different islands. The homogeneity is only moderate, but the orderliness is striking. For example, there are five genera of frogs and toads in the Greater Antilles, but only one genus (*Eleutherodactylus*) occurs on all four islands. This is a poor score for homogeneity. Every one of the five genera, however, is distributed in an orderly way, upon a series of adjacent islands or, if a single island, one which is adjacent to the mainland. The genus *Hyla* is found in Cuba, Hispaniola, and Jamaica, but not in Puerto Rico; *Bufo*, in Cuba, Hispaniola, and Puerto Rico, but not in Jamaica; *Leptodactylus*, in Hispaniola, Puerto Rico (and the Lesser Antilles), but not in Cuba or Jamaica; *Phyllobates*, only in Cuba. I have chosen frogs and toads as an example because they are well known and not too readily dispersed, and because they include no endemic Antillean genera. The evidence in some other groups is incomplete or complicated by doubt of the exact relationships of endemic forms. On the whole, however, in spite of a few exceptions, the distribution of related forms in orderly series is a striking characteristic of the Greater Antillean fauna.

The significance of this orderliness is, I think, very different from what has usually been supposed. The mere fact that order is present does not necessarily mean that dispersal has been across land connections. This has been pointed out in the first part of this paper, and some details of the orderliness of the Greater Antillean fauna will be discussed further below. What the orderliness shows most clearly is that the present Greater Antillean fauna has accumulated through series of immigrations at a time when the conformation of the land was not so very different from what it is now. Whether the immigrations were over land or over water is a question which may be postponed for the moment. The present fauna is a product of immigration, and there has been no wide-spread extinction of animals since the fauna originated. It is just about inconceivable that, if the present fauna were derived from a larger fauna of continental type by extinction of

many forms, the remnants would be distributed in such an orderly way, along what appear to be original migration routes. I think it is safe to conclude, then, that the Greater Antillean fauna is an *accumulation* of immigrants, not the *residue* of a larger fauna.

The terrestrial mammals seem at first to be evidence against this, for there is no doubt that many Antillean mammals have become extinct. A good many of them have become extinct only through human agency, however, and if all known forms, extinct as well as living, are considered, they are found to exhibit much the same moderate homogeneity and greater orderliness as the rest of the fauna. Ground sloths are known from Cuba, Hispaniola, and Puerto Rico, but not from Jamaica. Insectivora, too, are known from Cuba, Hispaniola, and Puerto Rico, but not from Jamaica; *Solenodon* is known only from Cuba and Hispaniola; *Nesophontes*, from all three islands. Rodents are more numerous and occur upon all of the Greater Antilles, but most of them are hystricomorphs of a few related groups. It is true that future discoveries of fossils may change this picture, but the facts as now known suggest that the mammals as well as the other Greater Antillean animals were, until the recent extinction of so many forms, distributed in an orderly way, and that the mammal fauna, too, was an accumulation of immigrants, not the residue of a continental fauna.

There seems, in fact, to be no significant exception to the orderliness of the Greater Antillean fauna. It should be noted especially that, although different animals have probably reached the islands at different times, the fauna as a whole does *not* divide into a group of older residual forms and a group of more recent immigrants. The frogs and toads are among the most ancient of existing land vertebrates—older in their origin than placental mammals—but no Greater Antillean animals are distributed in a more orderly way. If the frogs and toads are descended from a few immigrant genera, as their distribution seems to show, it is hard to believe that any Antillean vertebrates can be relicts of a continental fauna.

Irregular depauperateness of fauna

Island faunae probably always tend to be depauperate even on recent continental islands, for the size of an island probably limits to some extent the number of animals which can exist on it. The Greater Antilles, however, seem to lack many more forms of life than their size warrants. The depauperateness of the fauna is uneven, much more striking in some groups than in others. Several examples are given below. In analyzing them an attempt will be made to find whether or not the groups which are poorest in the Greater Antilles are those least likely to cross water barriers, and whether or not the groups which are best represented are those characteristic of oceanic islands.

Warm-blooded vertebrates. (I am indebted to Dr. G. M. Allen for reading and correcting this part of the paper.) Terrestrial mammals are very poorly represented on the Greater Antilles. The entire native fauna, including extinct forms, consists of two unrelated genera of primitive insectivores, three or four groups of primitive rodents, and ground sloths. One gets the impression from Matthew (1919, pp. 171–172) that this entire fauna might have been descended from six or seven ancestral immigrant types. It is impossible to say definitely from what direction the different forms reached the islands, for none has close relatives living anywhere on the main-

land. However, the insectivores probably came originally from North America; the sloths and rodents, from South America. The best guess probably is that they all reached the islands somehow by way of Central America.

Among the groups of animals which are absent (except for recently introduced forms) upon the Greater Antilles are perissodactyls, artiodactyls, proboscidians, carnivores, modern insectivores, and non-hystricomorph rodents, except that one of the latter (a species of *Oryzomys*) once occurred upon Jamaica and may have been endemic. Even more significant than the absence of these groups, most of which are of northern origin, is the absence of certain groups which have gone through long evolution in South America. For example, marsupials, anteaters, armadillos, the several peculiar groups of South American ungulates, and South American monkeys are completely unrepresented by either living or fossil forms on the islands, and even the hystricomorph rodents are represented by only a fraction of the groups which have existed in South America. (See Lydekker, 1896, Chapter 3, for an account of the history of mammals in South America.) There never was a time when free communication with either North or South America would have been expected to bring such a limited fauna to the Greater Antilles. A very large amount of fossil material—fragments of certainly thousands of individual mammals—has been recovered on the islands in cave and spring deposits of Pleistocene age. It is practically certain that there were no mammals larger than ground sloths and large rodents on the islands when these fossil deposits accumulated, and it is unlikely that many even of the smaller forms escaped preservation. Nothing is known, however, of the mammal fauna of the islands before the Pleistocene. Matthew (1918, 1919) gives further data and compares the Antillean mammal fauna with that of other islands.

According to Chapman and J. A. Allen (1893, pp. 231–234) the following terrestrial mammals occur on Trinidad, a recent continental island smaller than the smallest of the Greater Antilles, and only about 1/20 the size of Cuba: monkeys (2); carnivora (5 species, in 3 families); a squirrel, several mice, and other non-hystricomorphs as well as hystricomorph rodents; a deer; peccaries (2); the two-toed sloth; anteaters (3); an armadillo; and opossums (3). The presence of these animals on Trinidad shows that a relatively small island can support a much more diverse mammal fauna than is found on the Greater Antilles.

Bats, in contrast to the ground-living mammals, are relatively numerous and diverse on the Greater Antilles. According to Anthony's list (1926, pp. 208–211) the islands have 17 genera of bats in common with the mainland. There are also 10 endemic genera, most of which seem to be derived from mainland rather than from island forms. The minimum number of immigrant stocks necessary to have produced the Greater Antillean bat fauna is probably about 27, or about four times the minimum necessary to have produced the fauna of ground-living mammals. This is remarkable when it is remembered that bats are less numerous than ground-living mammals in continental faunae. It is true that it is impossible to be sure how many immigrations have really occurred in any group of Greater Antillean mammals, but it cannot be doubted that bats are disproportionately numerous and diverse and that they have reached the islands relatively often. Birds, too, are relatively numerous and diverse, so ob-

viously so that there is no need to cite figures.

G. M. Allen (1911) once tried to show that the bats as well as the ground-living mammals reached the Antilles over land connections. However, he did not make out a very good case, partly because he took the existence of local endemic forms as proof of complete isolation, partly because of the fact that bats are known to have reached various oceanic islands across water. (I learn from Dr. Allen that he now thinks that most of the Antillean bats may have been distributed over water.) Bond (1934) supposes that even most of the resident birds originally reached the Greater Antilles by land bridge via Central America. If this is so, it is a remarkable coincidence that birds, which are the only animals to migrate extensively to the Antilles from the north, show exceptionally strong North American relationships even among resident species. Did most warm-blooded vertebrates of all kinds reach the Greater Antilles by land from Central America? Or does the scarcity of flightless and the relative abundance and diversity of flying forms indicate immigration across water? For my part, I think that over-water immigration is indicated, and that the exceptional northern affinities of the resident birds indicate immigration of their ancestors from North America across the water gap. Remember that birds even more than bats have reached and populated oceanic islands.

Amphibians and reptiles. The following table gives the number of genera and species of frogs and toads, lizards, and snakes (turtles will be mentioned later) known from Cuba, Puerto Rico, and (for comparison) Trinidad, the only undoubted continental island of anything like comparable size in the Caribbean region. The figures for Cuba and Puerto Rico are from the latest West Indian checklist (Barbour, 1935); those for Trinidad, from Parker (1933, 1935) and Mole (1924).

	CUBA (AREA ABOUT 40,000 SQ. MI.)		PUERTO RICO (AREA ABOUT 3,400 SQ. MI.)		TRINIDAD (AREA ABOUT 2,000 SQ. MI.)	
	Genera	*Species*	*Genera*	*Species*	*Genera*	*Species*
Frogs and toads	4	22	3	13	12	23
Lizards	15	46	9	22	15	19
Snakes	8	14	4	6	28	38

Cuba is the largest and Puerto Rico the smallest of the Greater Antilles. The figures for Hispaniola (about 30,000 square miles) follow almost exactly those for Cuba, although some of the genera and most of the species are different; the figures for Jamaica (about 4,500 square miles) follow closely those for Puerto Rico.

This table shows that the amphibians and reptiles of the Greater Antilles are poor out of all proportion to the islands' size, especially when genera are considered. Species are relatively numerous, for endemic species have multiplied, especially on the larger islands. As a matter of fact even a few of the genera (of lizards and snakes) may be endemics derived from other Antillean forms. The number of original stocks necessary to have produced the Greater Antillean fauna may be even smaller than the figures show.

As compared with the fauna of Trinidad, that of the Greater Antilles is not only poor but is characterized by a predominance of lizards as compared with frogs and especially with snakes. On Cuba and Puerto Rico there are more genera of lizards than of snakes and amphibia together, while on Trinidad the *snake* genera outnumber the other two groups together. One or two of the Greater Antillean lizard genera may have been

introduced by man and one or two others may be endemic and derived from other island genera, which somewhat reduces the number of original generic stocks, but one or two of the snake genera may be endemic too. There is no doubt that lizards are disproportionately numerous and diverse on the Greater Antilles. Wallace (1876, p. 28; 1911, pp. 279–280) long ago pointed out that, for some unknown reason, lizards seem to be better able than frogs or snakes to cross ocean barriers, and more often occur on oceanic islands. The Greater Antilles, then, resemble oceanic islands not only in the limited nature of their fauna of amphibians and reptiles, but in the predominance of lizards.

Butterflies. Bates (1935, p. 83) states that there are 289 species of butterflies known from Trinidad, only 156 from Cuba. On p. 88 he says further, " . . . . exceptions seem to me of comparatively little weight when compared with the striking general occurrence of wide ranging, strong flying butterfly groups in the West Indies, and the absence of butterflies belonging to groups characterized by weak flying, non-migrating and local habit." This certainly suggests immigration of butterflies across a water gap wide enough to bar out weak flying, non-migratory forms, but, as Bates says, we do not know enough about the general habits of butterflies to draw final conclusions.

Fresh-water fauna. Briefly, some of the characteristics of the fresh-water fauna of the Greater Antilles are these: There is only one genus of fresh-water turtles, with several very closely related species. Some of the Antillean frogs have freed themselves from dependence upon open water by suppressing the tadpole stage in favor of direct development. This, however, is characteristic of frogs of some continental as well as some oceanic islands (Barbour, 1926, pp. 83–85). Strictly fresh-water fishes are not known at all from Puerto Rico, and those of Cuba and Hispaniola are descended from a very few stocks as compared with those of Trinidad.

My information about West Indian fishes is from letters from Dr. George S. Myers and Dr. Carl L. Hubbs, and from conversation with Dr. Thomas Barbour. Dr. Hubbs' letter is particularly quotable. He says, under date of April 6, 1937, "On the problem of the freshwater fish fauna of Puerto Rico, you should refer to the paper by Hildebrand in *Copeia*, 1935 (2). He shows that the fauna as listed comprises 2 freshwater fishes almost surely erroneously so recorded; several species introduced by man; the eel (1) (his species) which breeds in the sea; *Poecilia vivipara* (2) which also occurs through the Lesser Antilles to South America but is absent in Cuba, and which occasionally occurs in brackish or even salt water, though belonging to a freshwater family; *Agonostomus* (3) which occurs generally on isolated islands and presumably in some way passes through the sea; several gobies (4 to 9) that are more or less transitional between marine and fluviatile types. Of the gobies *Sicydium* ranks with *Agonostomus* in being an upland, swift-water inhabitant of isolated lands.

"The Lesser Antilles have virtually the same fish fauna as Puerto Rico. The description of a *Cichlasoma* from Barbados is now considered to have been based on a mislabeled Cuban specimen.

"Trinidad, in contrast, has a rather large freshwater fish fauna, of South American affinity, as long ago pointed out by Gill (*Ann. Lyc. Nat. Hist.* N. Y., 6:363–430).

"The Greater Antilles [excepting Puerto Rico] have a moderate freshwater fish fauna, in number of species, but one that is very depauperate in number of types. That is, in agreement with Dunn, we (Howell Rivero and I) have found and will show in a later paper that there is evidence for only a few migrations of freshwater fishes from the mainland (clearly from Central America) onto the Antilles. But several of the types after attaining the islands obviously underwent radiative adaptation, thus increasing the fauna in species and genera."

Among insects which live or breed in fresh water, stone flies (Plecoptera) seem to be completely absent in the Antilles.

There are some caddice flies (Trichoptera) and some May flies (Plectoptera) on most or all of the Greater Antilles, although the fauna of these orders is not rich. All of these groups are weak in flight. More strongly flying aquatic insects, such as dragon flies and water beetles, are usually better represented. Among mollusks, according to Mr. W. J. Clench, the fresh-water groups are very poor compared with those of the land, although the difference is due partly to the great multiplication of land forms.

On the whole, then, although information on the subject is far from complete, the fresh-water fauna of the Greater Antilles seems to be relatively poor. This may be due partly to the fact that, on some of the islands, there is little fresh water available except in small, rapid brooks. However, this is probably not the only factor involved. The adaptation of certain brackish-water fishes to fresh water, the radial evolution of some other fishes, and the relative abundance of certain strong-flying and readily dispersable aquatic insects suggest that the fresh-water habitat is adequate and that various aquatic animals are absent or poorly represented because of barriers to dispersal. Wallace (1911, pp. 304–305) and others have pointed out that paucity or absence of fresh-water life is characteristic of remote oceanic islands, and to a limited extent the Greater Antilles show this characteristic, whatever the reason may be.

Radiative evolution in significant groups

There is some question as to just how much radiative evolution has occurred in different groups of animals on the Greater Antilles. The terrestrial mammals may have evolved on the islands from only six or seven ancestors. Nothing is known of the history of their immediate mainland ancestors, however, so part of their evolution may possibly have occurred before they reached the islands. Dunn (1932) believes that the amphibians and reptiles of the Greater Antilles have arisen by radiation of species and even genera from relatively few immigrants, "extraordinarily few when one considers the enormous number of Central American genera"; but Dr. Barbour tells me that he believes that Dunn over-estimates the amount of radiation which has occurred actually upon the islands. Hubbs and Rivero (letter quoted above) believe that there has been extensive radiation among the fresh-water fishes of the Greater Antilles. And there has certainly been an enormous amount of radiation among the land mollusks. There has, I think, unquestionably been a good deal of radiation in all of these groups upon the Greater Antilles, in spite of details which are in doubt. However, it is hard to be sure just what this signifies. Radiative evolution occurs upon continents as well as upon islands, and amount of radiation must depend upon the age of an island's fauna as well as upon the mode of origin. Nevertheless, conspicuous radiation is probably especially characteristic of the faunae of oceanic islands, as Perkins (1913, pp. lii–lxi) has found it to be of the presumably oceanic Hawaiian fauna. The radiation which has occurred in the Antillean fauna, in conjunction with the other evidence, certainly suggests an over-water origin, but should not alone be given too much weight.

Significance of characteristics of fauna

The preceding discussion covers only a fraction of the Greater Antillean fauna, leaving most of the invertebrates out of

consideration, but the groups which have been discussed include the *best known* animals and those which, because of their large size and the difficulty which some of them have in crossing salt water, are most significant zoögeographically. These significant groups show that the fauna of the Greater Antilles has certain characteristics of the faunae of oceanic islands, at least in a modified form. Not only is the fauna depauperate, but the groups which are poorest seem to be those least likely to cross water barriers; many of the groups which are best represented are characteristic of oceanic islands; and there has been a good deal of radiative evolution in the most significant groups.

At first thought there seem to be three possible explanations of these facts. *First*, if the fauna of the Greater Antilles were the residue of a larger fauna of continental type, the present characteristics of the fauna might have arisen by extinction of animals unfitted for, and survival and multiplication of animals fitted for life on islands. There are, however, several objections to this. The orderly distribution of animals on the Greater Antilles, along what appear to be routes of immigration, makes it practically certain that the fauna is not a residual one. Comparison with continental islands such as Trinidad and the East Indies does not show that a fauna would be so highly modified by mere existence upon islands, although differences in age of different islands makes this evidence questionable. And the animals which are lacking or poorly represented on the Greater Antilles are in some cases of sorts that are well suited to existence in isolated habitats if they can get there. For example, sluggish, flightless insects are uncommon in the Antilles except for those which have lost the power of flight locally on certain mountain ranges (cf. Darlington, 1937), but flightless insects are well fitted for life on islands, on some of which they are very common.

Perkins (1913, pp. xlviii and following) mentions a large number of flightless insects which inhabit the Hawaiian Islands. Jeannel (1925) discusses some cases in which flightless beetles have survived successfully upon islands in the eastern Atlantic Ocean. Flightless birds, too, frequently occur on islands. There is every reason to think that sluggish, flightless animals are well suited to island life.

It appears, then, for various reasons, that the faunal peculiarities of the Greater Antilles are not the result of large-scale elimination of animals unsuited to island life.

Second, it might be supposed that migration over some special sort of land bridge has produced a fauna with the special characteristics observed. The difficulty is to say what sort of land bridge could produce the required result. If the fauna really has come over a land bridge, the latter must have been extensive enough and must have lasted long enough to carry fresh-water fishes and a variety of slowly moving land mollusks. No such bridge could have barred the many sorts of animals which are absent or poorly represented on the Antilles. A low, flat bridge like Florida would be no bar. A great many mammals, for example, have invaded the peninsula of Florida, including (according to the list published by Outram Bangs, 1898) the opossum, shrews, moles, various carnivora, diverse rodents, a deer, and (fossil) proboscidians, various ungulates, and other large forms. The amphibian and reptile fauna of Florida, too, has entirely different characteristics from that of the Greater Antilles. The numbers of genera of frogs and toads, lizards, and snakes in Cuba have been found to be respectively 4, 15, and 8; in Florida the numbers are 7, 8, and 25 (figures from

Stejneger and Barbour, 1933) and there are 13 genera of non-marine turtles in Florida as compared with 1 living genus and 1 extinct one (a giant land tortoise) in Cuba. (One genus of frogs and two of lizards which have been introduced into Florida are omitted from these figures.) There are in Florida also 10 genera of caudate amphibians, while the only Antillean record for the Caudata is now thought to be an error, but most of the Floridan caudates occur in the northern part of the state and have not penetrated the peninsula. A few genera of the other groups are limited to northern Florida, but the peninsular fauna is nevertheless much more diverse than that of the Greater Antilles. Moreover the snakes are dominant in Florida as they are on the continental island of Trinidad, while lizards are dominant on the Greater Antilles. Fresh-water fishes, too, are abundant and diverse in Florida. There is, then, no reason to think that a low, flat land bridge would have produced, by selection of immigrants, a fauna like that of the Greater Antilles. Nor is a mountainous land bridge (suggested by Schuchert, 1935, p. 110) more satisfactory. North and South America are now joined by a mountainous land bridge through Central America, and an enormous variety of animals is known to have crossed the bridge, including a great many mammals from proboscidia and large carnivora down. And how could a mountainous land bridge which could carry even a few fresh-water fishes to the Greater Antilles be supposed to bar anything else?

There remains the *third* possibility, that the Greater Antillean fauna is depauperate and shows some of the characteristics of the faunae of oceanic islands because it has been derived in the same way, across water. This, in my opinion, is the only reasonable explanation of the facts.

Distribution formulae

There is one other source of evidence on the mode of origin of the Greater Antillean fauna. It is the distribution and interrelationships of the animals on different islands. If land bridges have existed in the past, the present distribution of animals ought to reflect them; and if dispersal has been across water gaps, the distribution of animals ought to reflect that, too. It will be interesting, therefore, to try to decide what the relationships of the animals on different islands ought to be in different hypothetical cases, and to compare the results with the relationships which actually exist.

According to Schuchert (1935), if the Greater Antilles have been connected with the continent (and remember that Schuchert says, p. 110, that there is no geological proof that there has ever been a connection), the connection has probably been from Honduras to Jamaica, and has probably extended *by way of Hispaniola* to Cuba, or at least this has probably been the most recent series of connections.

What Schuchert actually says (p. 108) is, "As the writer sees the physical evidence, the Antillean basin broke down from the Gulf of Mexico across the Central American geanticline, first cutting off Cuba from Central America, then sending its waters east to the south of Cuba, next separating Jamaica from Cuba but not from Haiti, and eventually cutting Cuba off from Hispaniola; thus, the Honduras-Jamaica-Hispaniola bridge was the last part of the Antillean geanticline to break down."

If this has really been the geological history of the Antillean region, the relationships of the animals should be expressed by the following formula, the closeness of relationships being inversely proportional to the length of the dashes:

Honduras–Jamaica–Hispaniola—Cuba——Yucatan; Jamaica——Cuba

If, however, the relationships have been determined by the widths of existing water gaps (Fig. 4), the formula should be something like this:

Honduras —— Jamaica — Hispaniola-Cuba—Yucatan; Jamaica—Cuba

But the relationships actually shown by the most significant groups of animals, i.e. those, like mammals, amphibians and reptiles, and land mollusks, which are fairly well known and not too readily dispersed, are something like this:

Honduras —— Jamaica - Hispaniola - Cuba—Yucatan; Jamaica—Cuba

Certain details of this last formula are doubtful. There is, for instance, doubt as to whether Jamaica is really faunistically as close to Hispaniola as Cuba is. Anthony (1926, p. 198) says, "The mammalia of Jamaica is sufficiently aberrant in many details to warrant the assumption that it was probably split off from the main Antillean continent, if such existed, before the other three islands were broken apart." Possibly, however, the mammalian evidence (mostly fossil) is too incomplete to be trusted. The amphibians and reptiles were at one time considered to show a greater isolation between Jamaica and Haiti than between Cuba and Haiti, but Dr. Barbour tells me that recent discoveries indicate that Jamaica no less than Cuba is closely related to Hispaniola in its reptile fauna. Simpson (1894, pp. 434, 435, and 436) believed that Jamaica was shown by evidence of the land mollusks to be the most isolated of any of the Greater Antilles. This, however, was a long time ago; more complete collecting may show a closer relationship than was supposed between Jamaica and southwestern Haiti.

There is some doubt, too, as to whether the Cuban fauna is really closer than the Jamaican to that of the mainland. In amphibians and reptiles Cuba certainly has more groups in common with the continent (Barbour, 1914, pp. 224–225), but the evidence from other significant groups is not, so far as I have been able to find, very satisfactory.

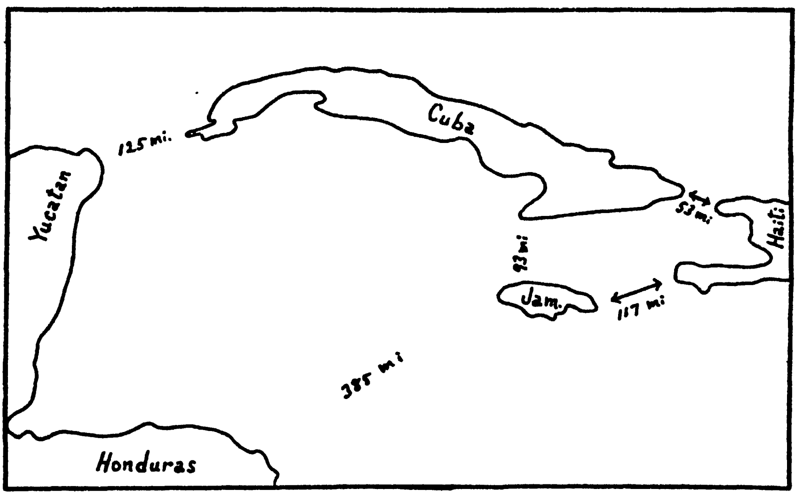

FIG. 4. OUTLINE MAP TO SHOW APPROXIMATE WIDTHS OF WATER GAPS IN WESTERN ANTILLEAN REGION

Arrows show direction of strongest faunal relationships. Note absence of strong relationships between Jamaica and Cuba, although water gap is narrower than that between Jamaica and Haiti.

In spite of doubt about details, there are two main facts expressed in the last formula which are beyond doubt. First, faunal relationships among the islands are definitely from Jamaica to Hispaniola to Cuba (Fig. 4), not directly from Jamaica to Cuba. Barbour (1910, p. 277; 1914, pp. 227–228) has pointed this out for the amphibians and reptiles; Bates (1935, p. 90) shows that the rule holds

remarkably for the butterflies; Mr. Clench tells me that it holds for the land mollusks; from my own experience I think it holds for the beetles; it appears to hold, with rare exceptions, for the whole fauna, except for some readily dispersed forms. The fauna shows an unmistakable Jamaica-Hispaniola-Cuba linkage. Second, the Greater Antilles are faunistically much more closely related to each other than any one of the islands is to the mainland; so far as I know, this is unanimously agreed upon by zoölogists.

As to interpretation of these facts, the definite Jamaica-Hispaniola-Cuba linkage almost certainly means either that the islands were at some time connected in this order or that the water gaps between Jamaica and Haiti and between Cuba and Haiti were relatively narrow. This faunal linkage is inexplicable by dispersal across present water gaps, but agrees very well with Schuchert's description, quoted above, of the probable geological history of the islands. On the other hand, the facts that the islands are faunistically rather homogeneous, that no one island has a fauna of anywhere near continental type, and that Jamaica is if anything faunistically more isolated than Cuba both from the other islands and from the mainland, and cannot possibly have been the center of dispersal of more than a fraction of the Greater Antillean fauna, exactly contradict Schuchert. According to the latter, the geological evidence is that, if there were ever land connections from the continent to the islands, the most recent was from Honduras to Jamaica. In fact this is said (p. 108) to be the only connection since the Triassic which is probable on geological grounds. Yet the distribution of the fauna does not reflect such a connection.

To give two examples more specific than those cited above, there are three orders of mammals (all placental mammals originated later than the Triassic) known from Cuba and Hispaniola, but only one of them, Rodentia, is known ever to have occurred in Jamaica. And of the five genera of frogs and toads in the Greater Antilles only two occur in Jamaica.

It cannot be said that the Antillean fauna is too mixed to show the direction of past connections, for it does plainly show the Jamaica-Hispaniola-Cuba linkage. I think that the conclusion must be that there was no connection with the mainland; that there was always a water gap at exactly the point where Schuchert (pp. 109–110) admits the geological evidence breaks down, in the region of the Rosalind and Pedro Banks. These banks were probably elevated, so that the water gap between Central America and Jamaica was not much wider than that between Central America and Cuba, for otherwise Jamaica would have received almost nothing directly from the mainland, but the banks probably never were connected to form a complete land bridge.

SUMMARY AND CONCLUSIONS

The first part of this paper deals, partly mathematically, with the chances of dispersal of organisms across water gaps and among islands, and concludes that, although the dispersal of individual organisms across water is partly accidental, the distribution of species which results is largely not accidental and not haphazard, but reasonably orderly, the order depending much more than is usually realized upon the relative widths of water gaps.

Next, the possibility of dispersal of animals across water from the mainland of America to the Greater Antilles is considered. It is found that the position of the islands, the geological history of the region, and the direction and nature of winds are such that animals may well

have been transported across water from the mainland to the islands by storms as well as in other ways, and it is found that the chances of such transportation have been best from the direction of Central America, the region from which most of the Greater Antillean fauna has actually been derived. Several common objections to over-water dispersal of Antillean animals are examined, and none is found to be a very serious objection. At this point in the paper it appears merely that animals *might* have reached the Greater Antilles across water gaps as well as across land connections. However, none of the evidence yet examined indicates how they really did reach the islands.

The last part of the paper deals with the actual characteristics of the Greater Antillean fauna. The fauna is found to be only moderately homogeneous but very orderly. The orderliness seems to be due to the fact that the animals are still distributed along the migration routes by which their ancestors reached the islands, and this is taken to show that the fauna is an accumulation of immigrants, not the residue of a continental fauna. As to whether the immigrants arrived over land or over water, there seem to be only two sources of real evidence. One is the nature of the fauna as a whole. The fauna is irregularly depauperate and shows various characteristics of an oceanic fauna, and the only reasonable explanation seems to be that immigration was across water gaps wide enough to bar out, partly or completely, those sorts of animals least able to cross water. The other is the distribution of animals among the islands. The distribution reflects no *specific* connection with the mainland, although it does reflect probable connections among the islands, between Jamaica and Hispaniola and between Cuba and Hispaniola. In particular there is no indication of a relatively recent connection from Honduras to Jamaica, although, according to geology, that is where the most recent connection with the mainland should have been, if there ever was a connection.

Both of the two independent lines of valid evidence indicate, then, that the fauna of the Greater Antilles has been derived from the mainland across water, although dispersal from one island to another may have been partly over land connections. Derivation of the fauna in this way not only accounts for its composition and for the way the animals are distributed among the islands, but agrees very well with what is really *known* about the geological history of the islands. So far as I can see, no other hypothesis will fit the facts.

In Fig. 5 is shown what seems to me the most probable conformation of the Antillean region when the bulk of the fauna reached the islands. Florida and Yucatan are shown submerged, as they are thought to have been during most of their history. The banks off Honduras and Nicaragua are shown not only elevated but extending much farther toward Cuba than they do now. So far as the distribution of animals goes, the approximation of Central America to Cuba might have been accomplished equally well by an elevation of the Campeche Bank off Yucatan, but the extension from Honduras is more likely on geological grounds. I should guess that the narrowest water gap toward Cuba was about 75 or 80 miles; toward Jamaica, slightly wider. I cannot justify this guess to the mile, but at least it seems likely that the gaps were narrower than the present Yucatan Channel, which is about 125 miles wide, while, on the other hand, they were wide enough to impress upon the fauna

of the islands some of the characteristics of an oceanic fauna. There may, of course, have been a series of two or more gaps instead of the single one shown. There may also have been narrow gaps between Jamaica and Hispaniola and between Cuba and Hispaniola, but these gaps, if they existed, were probably much narrower than the gap between Jamaica and Cuba.

And if hurricanes followed the same paths they do now, many of them would pass over the peninsula to the islands.

There seems, then, to be no good evidence for even one land bridge to the Greater Antilles. Yet if a land bridge theory were to be adopted at all, at least three bridges would probably be required, one to Cuba, another *independent* one to Jamaica, and a third from South America

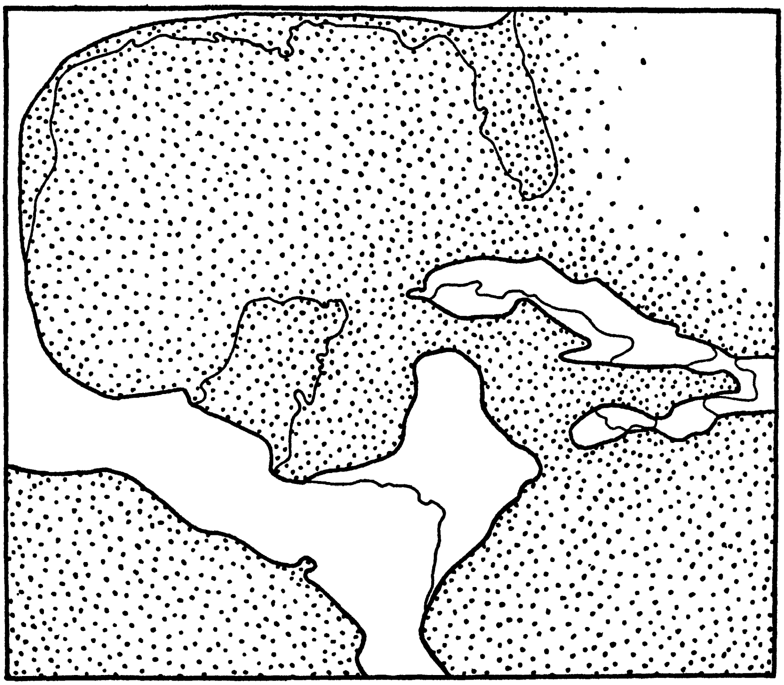

Fig. 5. Possible Conformation of Land (Heavy Lines) in Western Caribbean Region when most of the Greater Antillean Fauna Was Derived from the Mainland

Present land indicated with light lines. The Bahaman region has intentionally been left blank; it, and the Lesser Antilles, are outside the scope of this paper.

If the land did lie as shown in Fig. 5, chances of dispersal of animals across the water were probably very good indeed. The force of the trades would be broken by the union of Cuba, Hispaniola, and Jamaica, forming a gulf of relatively calm water favorable to rafting. The Gulf Stream might eddy or follow a course such as to carry drift from the Honduras-Nicaraguan peninsula to Cuba.

up the Lesser Antilles, for animals have certainly reached the islands from all these directions. (Several zoögeographers have claimed a fourth bridge direct from northern Africa to the Antilles, and at least one person has argued for a bridge from *Asia* to the Antilles but not touching either North or South America!) To give up any one of the three main land bridges would apparently be to admit

that certain mammals and reptiles, as well as other animals, have crossed water gaps, and once it is admitted that such forms as these cross water barriers at all, the chief argument for land bridges in the Antilles is gone. Any practiced land bridge enthusiast can draw a set of maps showing how a series of land connections might have produced the relationships shown by the Greater Antillean fauna. The difficulty is to match the connections with geological evidence, and to explain why, if there have been several connections with the mainland, the fauna of the islands shows so many characteristics of an oceanic fauna. It is hard to imagine even one land bridge sufficiently selective to have produced these characteristics.

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SYNCHRONOUS RHYTHMIC FLASHING OF FIREFLIES

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I. INTRODUCTION

ONE of the most interesting and complex types of group behavior in animals is that in which several organisms simultaneously repeat the same activity at regular intervals of time. In ordinary usage such behavior is called "synchronous," but, as pointed out by Craig (1916) and Alexander (1935), it actually involves two distinct factors, synchronism and rhythm.

Neither synchronism nor rhythm, alone, is uncommon. Synchronous behavior, for example, often occurs in a group of organisms as the result of a common response to the same stimulus. This stimulus may either be environmental, as in the synchronous responses observed in saw-fly larvae (Bennett, 1860), caterpillars (Beebe and Beebe, 1910), plant lice (Allard, 1917), fireflies (Allard, 1935), etc., or it may appear to originate in some individual in the group, as in the simultaneous behavior of birds (Annandale, 1900; Craig, 1916; Wheeler, 1917), prong-horned antelope (Wheeler, 1917), fish (Beebe, 1926), and frogs (Cunningham, 1903; Allard, 1917). Rhythm, likewise, is a common phenomenon in animals (Flattely, 1920), the chirping of crickets (Dolbear, 1897; Bessey and Bessey, 1898; Edes, 1899; *et al.*), and the beat of the heart being two examples among many. Lillie (1928) has pointed out several interesting analogies between physiological rhythms and those in inorganic systems.

The following pages deal primarily with types of behavior which involve both synchronism and rhythm. Such behavior has been reported in a wide variety of organisms, e.g., in ants (Peal, 1880, 1881; Forbes, 1881; Gounelle, 1900; *et al.*), Phalangidae (Newman, 1917; Wheeler, 1917), web-worm larvae (McDermott, 1916; Peairs, 1917), Orthoptera (Gould, 1895; Coues, 1895; Bostwick, 1895; Dolbear, 1897; Shull, 1907; Craig, 1916, 1917; E. S. Morse, 1916 a; Allard, 1917, 1918, 1930; Comstock, 1924; Fulton, 1925, 1928 a, 1928 b, 1934; *et al.*), caterpillars (Allard, 1917; Buxton, 1923; Minnich, 1925), bees (Burbidge, 1880; Evans, 1922), marine organisms (Herdman, 1903, 1903 a, 1905) wasps (Beebe, 1921), aphids (Williams, 1922; Tanner, 1930), termites (Conner, 1933), and many others. Since, however, by far the largest number of reports concern synchronous rhythmic flashing of fireflies, the subsequent discussion is confined primarily to these insects. There is presented a rather extensive review of the earlier literature, found in obscure journals and books of travel, and a more superficial review of the more recent literature, found in accessible journals. Among the many friends who have aided in the preparation of this paper, I wish in particular to thank Dr. E. W. Gudger who generously placed at my disposal a large number of references to synchronism.

The earliest account found is in Kaempfer's (1727) description of a trip down the Meinam River from Bangkok (Siam). He says:

The Glowworms (*Cicindelae*) represent another shew, which settle on some Trees, like a fiery cloud, with this surprising circumstance, that a whole swarm of these Insects, having taken possession of one Tree, and spread themselves over its branches, sometimes hide their Light all at once, and a moment after make it appear again with the utmost regularity and exactness, as if they were in perpetual Systole and Diastole.

Turpin, in a book on Siam (1771) says (translation): "Nothing gives a finer sight at night than to see a tree all covered with fireflies; it seems decked with many bright sparks which go out and rekindle almost at the same moment."

Goldsmith (1811) states: "The trees on the banks of the Meinam are finely illuminated with swarms of fireflies, which emit and conceal their light as uniformly as if it proceeded from a machine of the most exact contrivance." (See also Van Vleck, 1924.)

Bishop Pallegoix (1854) reports (translation):

The fireflies which are found in Siam are very common and are remarkable for the intensity of their light. One sees these insects fly separately from one tree to another and in all directions, but more often they are assembled by thousands on a great tree on the bank of the river. It is a magnificent spectacle to see spring out at one time from all the branches of this tree as it were, thousands of great electric sparks, because these fireflies emit not a continuous light but one interrupted by the effect of a sort of respiration. It is difficult to explain how this emission of light is simultaneous for several thousands of individuals.

Sir John Bowring (1857) poetically remarks of the Siamese firefly: "They have their favorite trees, round which they sport in countless multitudes, and produce a magnificent and living illumination: their light blazes and is extinguished by a common sympathy. At one moment every leaf and branch appears decorated with diamond-like fire; and soon there is darkness, to be again succeeded by flashes from innumerable lamps which whirl about in rapid agitation."

Cameron (1865) describes observations made in Burma:

The bushes literally swarm with fireflies, which flash out their intermittent light almost contemporaneously; the effect being that for an instant the exact outline of all the bushes stands prominently forward, as if lit up with electric sparks, and next moment all is jetty dark—darker from the momentary illumination that preceded. These flashes succeed one another every three or four seconds for about ten minutes, when an interval of similar duration takes place; as if to allow the insects to regain their electric or phosphoric vigor.

A very interesting discussion of Cameron's report is found in *The Transactions of the Entomological Society, London*, Ser. III, vol. 2, pp. 94–95, 1865, in which the Rev. H. Clark reports having seen synchronous flashing in the Organ Mountains (Brazil?).

Theobald (1866) gives a full description of a display witnessed along the banks of the Irawadi Delta in Burma; which is quoted here in part:

The bushes overhanging the water were one mass of fireflies. . . .The light of this great body of insects was given out. . . .in rhythmic flashes, and for a second or two lighted up the bushes in a beautiful manner; heightened, no doubt, by the sudden relapse into darkness which followed each flash. There are the facts of the case (and I may add that it was towards the end of the year) and the only suggestion I would throw out, to account for the unusual method of luminous emanation, is that the close congregation of large numbers of insects, from the small space afforded them by the bushes in question, may have given rise to the synchronous emission of the flash by the force of imitation or *sympathy*.

Collingwood (1868) says, concerning fireflies at Singapore:

. . . .on fine evenings and in favorable (that is, damp and swampy) localities, they present a very remarkable appearance. Clustered in the foliage of

the trees, instead of keeping up an irregular twinkle, every individual shines simultaneously at regular intervals, as though by a common impulse; so that their light pulsates, as it were, and the tree is for one moment illuminated by a hundred brilliant points, and the next is in almost total darkness. The intervals have about the duration of a second, and during the intermission only one or two remain luminous.

Burbidge (1880) also describes a display near Singapore: "As we glided onwards their numbers increased, until we came upon them in thousands, evidently attracted by some particular kind of low tree, around which they flashed simultaneously, their scintillating brilliancy being far beyond what I could have imagined to be possible."

Severn (1881), referring to an Indian firefly, says:

The curious pulsation of flashing of their light is remarkable, the insects on the tree all act in perfect concert, i.e., five seconds of no light, then seven rapid flashes; five seconds no light, seven flashes; and so the game continues throughout the dark hours. It is also worthy of special notice that all the glow-insects on a dozen or more trees will continue to keep up the most perfect time to the flashing of their light and the interval of pause, and this for many consecutive hours; but this singular agreement as to the time relates to close clusters of trees only. Thus distinct groups of trees separated by one or more hundred yards may not agree, and do not do so as a rule.

Annandale (1900) has given us a rather detailed and careful description of a display witnessed in Malaya:

A large tree was covered with many hundreds of fireflies, the majority of which seemed, judging from the similarity of their lights, to belong to one species, or perhaps to one sex. There were three individuals seated together, however, whose lights were larger and bluer than those of the others. The lights of all the specimens of the more abundant variety flickered in unison with one another; those of the minority, the three individuals, flickered together also, but in different time. At one instant the tree was lighted up as if by hundreds of little electric lamps; at the next it was in complete darkness except for three blue points.

Cunningham (1907) says, in reference to Bengalese fireflies:

As the train panted slowly upwards, many of the trees alternately flamed out into dazzling splendor and vanished off in the gathering gloom of an impending storm, whilst hosts of insects resting in them lit and put out their lamps as though by common consent. The cause for such simultaneous action on the part of countless individuals is hard to imagine, but there can be no question of the fact that such displays do take place.

Shelford (1916), referring to a display in Borneo, says: "On the opposite bank was a small tree growing close by the water's edge, which was covered with thousands of Fire-Flies, small beetles of the family *Lampyridae*; and I observed that the light emitted by these little creatures pulsated in a regular synchronous rhythm, so that at one moment the tree would be one blaze of light, whilst at another the light would be dim and uncertain."

Wells (1924, 1925) describes a rather astonishing manifestation of synchronism in Malaya, although it is not clear whether this display involved rhythm also. He says:

About every fifteen minutes these flies separated into two armies, one settling on the trees growing on the left bank of the river and the other on the right bank. Then, when I had decided that they had gone to bed for the night, the whole army on the left bank gave one big flash in perfect unison, which was immediately answered by another big flash from the right bank. There must have been thousands of them stretching along the river bank for a hundred yards or more, but the flies at one end of the line flashed their lights exactly at the same time as the flies on the other end. . .

Guenther (1931) reports witnessing a display in Brazil. He says: "In Petropolis . . . I noted how hundreds of green lights blazed out *simultaneously* and were simultaneously extinguished; with so regular a rhythm that it seemed as though the sparks were blown upon by a huge

mechanical bellows that gave a puff every second."

To pass more cursorily over the more accessible recent reports, rhythmic synchronous flashing of fireflies has been observed in the Philippines by Cox (1917), Purssell (1918), Barnes (1919), F. Morse (1918), and Brokenshire (1929); in New Guinea by Dodd (1918); in Siam by Reinking (1921), Morrison (1927, 1929), Smith (1935), and Alexander (1935); in Jamaica by Miller (1935); in India by Connor (1933); in Mexico by Merril (1930); in Borneo by Muir (1908, 1916); and in this country by E. S. Morse (1916), Allard (1916), Hudson (1918), Snyder and Snyder (1920), Hess (1920), and Rau (1932). In addition, there have appeared a considerable number of more or less circumstantial second-hand reports from various parts of the world. Some of these are cited in the reviews of Blair (1915), E. S. Morse (1916 a, 1916 b, 1918, 1924), Craig (1916), Allard (1917), Dahlgren (1917), Gudger (1919), Williams (1922), Allee (1927, 1931), Howard (1929), and Crawford (1934).

Of the 36 reports of synchronous flashing discussed above, 15 are from the Malay Peninsula (Siam, Burma, Malay States, etc.), 5 from the Philippines, 3 from Bengal, 3 from the East Indies, 6 from the United States, 2 from Brazil, and one each from Mexico and Jamaica. The preponderance of reports from the Oriental region (26) as compared with those from the rest of the world, is very striking.

The evidence presented leads to the following conclusions:

(1) It is reasonably certain that rhythmic synchronous flashing of fireflies occurs.

(2) Nearly all the Oriental reports state that the synchronism involved huge numbers of insects; that the displays occurred on trees growing in or near water; that synchronism is common in some regions and species; and that a given display persists unbroken sometimes for hours. The reports from the United States, markedly fewer and less consistent than those from the Orient, give the impression that synchronous flashing is exceedingly rare.

(3) In some displays observed the synchronism was reported to be "perfect," i.e. all the insects flashed at precisely the same instant (Kaempfer, Goldsmith, Bowring, Burbidge, Annandale, E. S. Morse (1916 a), F. Morse, Reinking, Wells, Morrison, Brokenshire, and Smith). In others the synchronism was not perfect: it either began at one or several foci among the aggregated insects and swept rapidly over the whole assemblage ("wave synchronism," Hudson, Hess, Williams, and Alexander), or a few insects flashed in the "dark" periods between mass synchronous flashes (Collingwood, Annandale, Purssell, Barnes, Hess, Miller, and Smith). Annandale, Hess, and Smith, however, maintained that the asynchronous flashes were produced by individuals of a different species, and Morrison suggested that they were due to the residual glow of the light organ after the synchronous flashes.

II. INTERPRETATIONS

The theories propounded to explain synchronous flashing are nearly as numerous and remarkable as the reports of the displays themselves. Some of these theories deal with synchronism, some with rhythm, and some with both. It will be convenient to separate them into five groups.

Obviously inadequate explanations

Included under this heading are the suggestions that puffs of wind influence

the insects alternately to expose and conceal their lights (*Trans. Ent. Soc. Lond.*, 1865); that synchronism is due to twitching eyelids (Laurent, 1917); and that the synchronism is in some way connected with the sap of the trees on which the insects gather (Morrison, 1927).

Accident

The accident theory is based upon the common observation that fireflies tend to flash rhythmically and that the rhythm is of approximately the same frequency in all the individuals in a given region. The argument then is that if the fireflies once get into synchronism with one another they will continue to flash synchronously because of their equal rhythmicities. The supporters of the theory are thus concerned with suggesting ways by which the original synchronization might accidentally be brought about. Gates (1917), however, apparently believed not only that the first synchronous flash occurred entirely by accident, but, what seems statistically even more improbable, that each succeeding rhythmic synchronous flash also was accidental.

Several authors (Allard, 1916; Hudson, 1918; Rau, 1932) have noted that some displays of synchronism take place under unusual environmental conditions, such as exceptional humidity, calm, darkness, etc., and in a large open space crowded with insects. Snyder and Snyder (1920) maintain that such conditions insure the uniformity of the natural rhythmicity of flashing so that synchronism, once initiated, will continue. No mechanism for initiating synchronism, however, is suggested. McDermott (1916) and Rau (1932) similarly regard synchronous flashing as a rare phenomenon brought on by a fortuitous concatenation of atmospheric factors. Rau offers, for the display he observed, the following tentative explanation: "Perhaps the entire population was ready to rise in flight but was held back by the shower; when this suddenly ceased, they were all in equal readiness, and at the propitious moment took to wing and flashed together. Since their flashes are at a fairly uniform interval, they continued in unison quite by accident, and did not break step for a few minutes."

The "accident" theory would account for "perfect" synchronism, but not for "wave" synchronism. It should be noted, however, that the definition of synchronism is an academic question: The percentage of a population which must be performing a rhythmic action simultaneously in order for it to be called synchronous doubtless varies greatly depending on the type of behavior and the frequency of the rhythm. "Wave" synchronism, however, is fundamentally incompatible with "perfect" synchronism, since it seems clear the the former must involve a response of each individual to the flash of a near neighbor, so that the community flash occurs progressively, like an ignited gunpowder train, and it is equally clear that the latter could not be brought about by such a mechanism.

Whereas special environmental conditions might conceivably induce isolated instances of synchronism, it seems unlikely that they can account for the spectacular displays exhibited by Siamese fireflies, in which Smith (1935) asseverates that the synchronism continues hour after hour each night for months, "without regard to air currents, air temperature, moisture or any of the other meteorologic conditions which have been stated to influence firefly flashing." Moreover, it is open to question whether the basis of the accident theory, namely, that the synchronism, once initiated, would continue autonomously, is tenable. Thus Snyder and Snyder (1920)

and Buck (1937c) have shown in an American species that there is a normal individual variation of from five to ten per cent in the mean duration of the rhythmic interval between successive flashes, even under uniform conditions, so that these individual variations would in all probability very soon carry the various individuals entirely out of phase with one another and disrupt the synchronism.

Illusion

Bates (1865) first suggested that synchronous flashing is an illusion. This notion has been strongly championed on psychological grounds by Craig (1916, 1917) and Ruckmick (1920). Craig contends that all synchronous rhythmic activities depend on a (perhaps unconscious) learned "conceptual awareness of the relation between one's own actions and the actions of others, and purposive imitation of the latter." This faculty, he says, is present only in man, although lower animals may exhibit an admirable synchronism (not rhythmic), which, under special conditions, may coincide with some powerful environmental rhythm. Craig then argues that the reports of synchronous flashing are illusions due to a "predisposition to perceive rhythm" in the accidental synchronism statistically to be expected in aggregations of flashing fireflies.

This thesis of illusion is amplified from the standpoint of experimental psychology in a remarkable contribution by Ruckmick (1920). From a study of the published reports Ruckmick concluded that most of the observations were made in an emotional attitude "bordering on the romantic" and that their accuracy was thus subject to suspicion. Supposing, then, that several coincidences should take place in a large number of flashing fireflies, the "well-known tendency of the human mind to integrate its experiences" would, according to Ruckmick, "tend to set the mind of the observer in the direction of subsequent groupings of the flashes into patterns, supplied, for the most part, by himself." Ruckmick accordingly tested this hypothesis in the darkroom, by exposing subjects to numerous electric lights flashed by machinery in a disorganized manner (no pattern or rhythm). These experiments resulted, first, in spontaneous (unsolicited) comments that the flashing lights reminded the subjects of fireflies flashing, and second, in likewise unprompted groupings of the flashes into spatial, and more especially, temporal patterns, the latter being described as "rhythmic." Ruckmick's conclusion is that under conditions simultating the natural there is a strong tendency for the mind to read order into disorganized experiences, and on this account reports of prolonged synchronous flashing are likely to be unreliable.

It seems unlikely that the evidence and arguments advanced by Craig and Ruckmick, cogent as they are, can serve to invalidate the multitudinous reports of synchronous flashing, many of which were made by experienced and careful observers, but at any rate there are at least two questionable features in Ruckmick's work: (1) His premise that not infrequently all the fireflies in a given large group would accidentally flash in unison is statistically very improbable. (2) Ruckmick's experimental setup was not an accurate imitation of the flashing of fireflies in nature, in that his lamps were not flashed rhythmically; whereas actually most of the fireflies in a given region flash rhythmically and with very nearly the same period (Snyder and Snyder, 1920; Buck, 1937 c). Moreover, the present writer was unable to detect any synchronism in the flashing of huge ag-

gregations of tropical fireflies, in which each individual flashed with the same rhythm, but quite at random with respect to the others (Buck, 1937 b).

Sense of rhythm

Bowring (1857), Theobald (1866), and Wheeler (1917) consider the synchronous flashing to be due to a "sympathy" or "Einfühlung" among the insects. This principle apparently corresponds to the "conceptual awareness" of Craig, already discussed, and to the "organic law of rhythmic appreciation" of Allard (1935). This idea involves consciousness of one's own rhythm, consciousness of one's neighbor's rhythm, and deliberate imitation of the latter; or, in anthropomorphic terms, a "sense of rhythm."

The "sense of rhythm" or "sympathy" hypothesis is not so much an attempted explanation of synchronous flashing as it is a more or less gratuitous attempt to relate the phenomenon to similar behavior in higher animals and man. The reason for this attempt is, apparently, the conviction that displays of synchronism in which all the fireflies flash exactly in unison cannot be understood without postulating the existence in insects of some faculty similar to that which enables human beings to walk or sing in unison. It is, however, possible to explain such apparently exact synchronism in a simpler and more objective manner. Such an explanation is discussed below.

Leader or pacemaker

The most popular proposed explanation of rhythmic synchronism is the "leader" or "pacemaker" theory, espoused by Blair (1915), Allard (1917, 1918), Hudson (1918), Hess (1920), and Alexander (1935). The essential feature of this theory is that all the fireflies simultaneously respond to the flash of one (the leader), which flashes rhythmically. This theory is an improvement over the accident theory in that it provides a mechanism for keeping the flashes synchronous so that the normal individual variations in rhythm will not break up the synchronism, but it is nevertheless open to criticism on the following points:

(1) Most of the proponents of the leader theory maintain that the flashes take the form of a wave, whereas a communal response to a single leader would necessarily result in perfect synchronism.

(2) As pointed out by Annandale and by Morrison, a single leader could not possibly be visible to all of the members of such huge swarms of fireflies as are reported to exhibit synchronism. (The fact that the compound eyes of insects are situated on the sides of the head, and designed for perception over a very wide angle, makes this a less serious objection than it would be if applied to primates, where the field of view is greatly circumscribed. Furthermore, since so many of the reports emphasize the wetness of the localities in which the displays of synchronous flashing occur, it might be imagined that the flash of the leader could be relayed to the rest of the swarm by means of its reflection on the water below.)

(3) No mention is made of what determines which individual shall be the leader, or of why he should flash more regularly than the others. Unless the leader's flash is somehow different from all the others (of which there is no evidence) he would have to flash at a slightly faster rate than any of the other fireflies to prevent being anticipated by some individual which happened to flash a little sooner than the average rhythm interval. Moreover, if each collective flash depended on the leadership of a single individual the whole group would re-

main dark if the pacemaker missed a single beat.

(4) The contention of Alexander (1935) that "in the absence of a pacemaker mechanism we should be forced to postulate the existence of an accurate physiological chronometer, a mechanism to most of us quite inconceivable," seems scarcely germane, since it is precisely such a mechanism which must activate the "pacemaker" itself. Moreover, not only are we quite familiar with such chronometers, or their manifestations in higher animals (e.g., heartbeat, action potentials in nerve, peristalsis, respiratory rhythm, etc.; cf. Child [1924] pp. 184–186) but there is good reason to believe, from the work of Brown and King (1931), Snell (1931, 1932), and others, that such a mechanism exists in the firefly. Snyder and Snyder (1920), indeed, have compared the flashing rhythm to the rhythmic electrical discharges originating in the central nervous system of vertebrates. Further data on the physiology of pacemakers are contained in Hoagland's (1935) book.

Richmond (1930) has suggested a simplified form of the leader theory, which, because it eliminates the idea of a single fixed pacemaker, obviates some of the criticisms presented above. Richmond's theory postulates first, that all the fireflies flash rhythmically, with nearly the same period, due to the alternate discharge and recovery of some battery-like mechanism (cf. also Blair), and second, that each insect flashes immediately if stimulated by a flash which occurs at a time near that at which he would normally flash. The title "leader" thus would apply merely to the firefly which happened to flash first after the last concerted flash.

Richmond's postulation is supported by certain analogies in nerve physiology. If, for example, the flashing is under nervous control (which seems certain from the work of Lund [1911], Brown and King, Snell, *et al.*), it might be expected that following each flash there would be a "refractory" period during which the insect would not respond to the stimulus of a flash, and during which he would not flash of his own accord (inhibition?). After this period a potential of some sort would be built up which would result ultimately in a spontaneous flash, but which could be discharged meanwhile by the stimulus of a flash from some other individual (cf. Allee, 1931, p. 92, and Child, 1924, pp. 276–287). Certain experiments of Hess (1920) and Buck (1937 c), described below, indicate that a mechanism similar to that here postulated may be present in the firefly, although Snyder and Snyder assume that at a given temperature "the flashing mechanism discharges [spontaneously] as fast as it can." That the battery analogy is not an accurate one in the sense that it implies that some substance is used up during the flash, and another flash cannot occur until more substance appears, is indicated by the work of several investigators who induced fireflies to flash at a rate higher than normal by electrical stimulation, or treatment with chemicals.

Summary

It will be seen that none of the theories proposed to account for synchronous flashing of fireflies provides an adequate explanation of the phenomenon. Richmond's modified leader theory appears in general to present the fewest intrinsic difficulties, although, like the other theories, it is mainly speculative. It provides a reasonable explanation for perfect synchronism within a relatively small area, by postulating that all the fireflies in the area respond synchronously to the first individual within sight which flashes after the last preceding synchro-

nous flash. It also can be adapted to explain synchronism over a larger area, providing that each synchronous flash passes as a wave over the assembled fireflies, by postulating that each firefly responds not to one particular individual, but to the flash of some member of the swarm, and that he, in turn, stimulates another, and so on.

Richmond's theory, however, like all the other theories, appears to be completely inadequate to explain displays of synchronism of the magnitude observed in Siam by Morrison (1927, 1929) and Smith (1935), who report that enormous numbers of fireflies, distributed along a half-mile of river front, all flash in exact unison. If the synchronism is really as absolute as reported, there seems to be no obvious escape from the necessity of referring the phenomenon to some abstract faculty like "sense of rhythm," since all the individuals obviously could not be responding to a single leader. However, the frequency of the flashing rhythm is reported to be very high (more than a hundred times per minute, according to several observers), and this ought to make it very difficult to distinguish between absolute synchronism and a very rapid wave flash of the type described by Alexander (1935), presumably in the same fireflies studied by Morrison and Smith. Assuming that the synchronism is in fact of the wave type, the Siamese displays are amenable to a simpler explanation than the above, by means of the Richmond leader theory. The reaction time between the flash of the leader and the response of the other fireflies, however, would have to be exceedingly short.

III. EXPERIMENTAL

Introduction

Gates (1917) was unsuccessful in attempts to produce synchronism or to alter the frequency or manner of flashing in *Photuris pennsylvanica* by means of a flashlight.

Morrison (1927, 1929) states that he inhibited the synchronous flashing of fireflies in a tree "by exposing them to a bright light for about a minute," and that "when the light is turned off the synchronism returns, having its origin apparently in some individual or group generally located in the central part of the tree."

Hess (1920) maintains that in observations on synchronous flashing in *Photinus consanguineus* he discovered that "by standing on the side of the valley and causing short flashes with a pocket flashlight, the fireflies of the entire valley responded," and that by flashing slightly sooner than the normal time for flashing he could increase the frequency of the synchronous flashes to some extent.

Allard (1935) induced several females of *Photinus scintillans* to respond synchronously to flashes from a flashlight. This has been observed many times by the present writer in females of *Photinus pyralis*, but since in both species the responses occur only following a stimulus they are not truly rhythmic.

Relation between the mating signals and synchronous flashing

It has been demonstrated (Buck, 1937 c) that the male and the female of the American firefly, *Photinus pyralis* are brought together for mating by a system of flashing signals of which the essential feature is that the responding female invariably flashes two seconds after each successive rhythmic flash of the male.

Earlier (Buck, 1935) it was shown that during the mating signals several males are often attracted simultaneously to the same female, and that these males flash in unison. It was further shown that synchronism on a larger scale can readily be induced by substituting a flashlight for

the female in the mating signals, and a theory to explain how such synchronism could spread to include a large number of fireflies was presented, as follows: "A little group of synchronously responding males built up around one female, as described above, acts as a unit in stimulating another female a considerable distance away because the combined intensity of the several simultaneous flashes is greater than that of a single flash; the second female, then, in responding to the first group of males, gathers in to herself a coterie of males which flash in unison and are, of course, synchronized with the original group; they in turn stimulate a third female which "attracts" a third cluster of males, also synchronized with the original, and so on, until a large number of fireflies scattered over an extensive area are flashing in unison. The whole process depends on the fact that all the females reply to each of the flashes of the male at the same definite interval."

This theory is a reasonable explanation of synchronism in *P. pyralis*, but does not account for the synchronous flashing observed by Hess (1920) in *P. consanguineas*, if his contention that both males and females flash synchronously together is valid. In addition, Miller (1935) and Smith (1935) assert that it does not account for the displays which they saw in Jamaica and Siam, respectively, because no females were seen. Smith in fact maintains that the females remain in the jungle from which the male fireflies "have definitely flown for the purpose of engaging in this nightly display."

Relation between subjection to darkness and synchronous flashing in P. pyralis

Males of *P. pyralis* were put into a large mosquito-netting cage in the darkroom and subjected to a light intensity of about 65 meter candles (for details see Buck, 1937 a). It was found that sooner or later, depending on previous treatment, these fireflies began to fly about and flash in a manner typical of their behavior in the field. Under these circumstances, their flashes, although rhythmic for a given individual, showed no sign of synchronism. If these males were then suddenly subjected to total darkness the character of the flashing was immediately and strikingly altered. All the fireflies flashed synchronously: then this fulguration was followed by four or five seconds of complete darkness, after which another salvo of flashes ensued. This rhythmic synchronism persisted uninterrupted for a considerable time and then gradually died away. Careful observation showed that each community flash was initiated by a single flash, the others following almost instantaneously. Each individual apparently took his cue to flash from his more immediate neighbors, so that the mass flash took the form of a very rapid chain of overlapping flashes, the insect farthest from the "leader" flashing last, but so promptly as usually to light up before the leader's flash had faded. That the synchronism was truly due to a follow-the-leader system and not to accident or individual adherence to some fixed rhythm is shown by the following facts: (1) Different individuals acted successively as the leader, the first flash after the dark between period being the one followed, regardless of its source; (2) There was considerable variation in the time between successive concerted flashes during the later, more irregular phases of the process, in which as much as ten seconds sometimes elapsed before the signal for flashing was given. That the synchronism was not induced specifically by the unnatural conditions of complete darkness is indicated by the fact that the same phenomenon was occasionally seen in the field among males which were sufficiently close together. The

total darkness thus seemed to serve only to increase the relative intensity of the flashes of the neighbors.

This synchronizing tendency in the males of *P. pyralis* probably could not in itself produce a mass synchronism among large numbers of specimens in the field, such as the display reported by Rau (1932), unless the insects were extremely crowded, and then the synchronism would be expected to take the form of waves spreading out from one or several foci. However, it may be the explanation of how several males which respond to the same female get into synchronism with each other (Buck, 1935).

The responses of the male of *P. pyralis* thus appear to be of two types: (1) To a stimulus occurring approximately two seconds after his flash he responds by orienting and proceeding toward the lure, whether female, flashlight, or another male (Buck, 1937 c). (2) To flashes which are close enough to him, i.e. of sufficient relative intensity, and which occur near the time at which he would be expected to flash, he responds by flashing synchronously with them (cf. Richmond, 1930).

IV. SUMMARY

The evidence presented in the preceding pages indicates clearly that rhythmic synchronous flashing of fireflies is a very complex phenomenon, and that it differs greatly in different localities and species.

The four principal theories formulated to explain synchronous flashing are analyzed in detail: (1) The "accident" theory, which postulates that synchronism is initiated by accident, and maintained by the normal rhythm of flashing of the members of the swarm. (2) The "illusion" theory, which holds that the human mind deceives itself into seeing rhythmic synchronism in the few accidental coincidences which would (allegedly) be expected to occur in large numbers of fireflies flashing at random. (3) The "sympathy" theory, which attempts to relate synchronous flashing to the human "sense of rhythm." (4) The "leader" theory, which maintains that synchronism is produced by the mass of fireflies responding to the flash of a leader.

None of these theories is adequate to explain fully all the features of synchronous flashing observed. In general, a modified form of the "leader" theory appears to be the most satisfactory.

Experimental evidence is presented which indicates that in the American firefly, *Photinus pyralis*, synchronism is sometimes a by-product of the flashing signals used in mating, and is sometimes due to a follow-the-leader response.

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SOCIAL FACTORS IN THE ORIGIN OF DARWINISM

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THE promulgation of the doctrine of evolution in the publication of *The Origin of Species* was a profoundly important event not merely for biology but for society as a whole. The social implications of Darwin's work concerning, for example, the origin of man or the doctrines of *laissez faire* and "progress," were disturbingly pertinent to the fundamental religious and socio-economic issues of mid-19th century life. Indeed, the acceptance of *The Origin of Species*, even in the biological world, was expedited by the relevance of the subject of evolution to the general intellectual temper of the day.

This impact of evolutionary thought on 19th century culture is an oft-told story. But it does not seem to be as generally recognized that the origin of Darwinism was in many ways an outgrowth of social conditions similar to those it affected. It is the purpose of this essay to discuss certain aspects of this relation, to trace the influence of some of the more important non-biological social factors on the maturation of the doctrines of evolution into what became known as Darwinism.

I

Darwinism is here understood to mean the body of fact and theory bearing on the general problem of the origin of species presented by Charles Darwin in his works *The Origin of Species* and *The Variations of Animals and Plants under Domestication.* It is in these publications that we find the first extensive proof that organic evolution is a fact, and that natural selection is the mechanism of evolution. In tracing Darwinism, in this sense, to its social roots we are dealing with a problem of contemporary interest. For Darwinism is still one of the most outstanding and fundamental truths of biology; and it is generally admitted to-day that the outline of evolution as given in the above mentioned works still holds. Furthermore, Darwin's work included so many fundamental divisions of biology that are still being actively investigated, that a study of its social origins is in considerable measure a study of social factors that affected the development of the whole of modern biology.

Darwin developed his proof that species originate by evolution from the facts of natural history, distribution, comparative anatomy, geology and paleontology, and variation and selection. His apprehension of natural selection as the mechanism of evolution was based on a reading of Malthus' book on the principle of population. The task of demonstrating social origins in Darwinism therefore reduces to revealing the influence of relevant social factors in the development of natural history, distribution, comparative anatomy, geology and palaeontology, variation and selection, and Malthusianism.

II

The first three of these subjects form a rather well-defined unit, for they grew together as the knowledge of organisms

was extended to include species from all parts of the world. The problem of social influences in the growth of this unit is so extensive that it can be treated here only in broad outline. At the close of the Dark Ages (*ca.* 1000) the formal knowledge of organisms was conditioned by ecclesiastical limitations; there was a widespread and naive faith in the dogma of the universality and eternity of the commonly known species, mostly European, and in the existence of a host of mythological creatures; and there was a blind acceptance of the story of the special creation of all living things by Divine Fiat given in the *Book of Genesis*. These beliefs were gradually broken down as new information about plants and animals poured into European centers with the return of travelers to distant lands.

The first of these movements of major significance was the Crusades (11th–13th centuries). These expeditions were only in part motivated by religious zeal. Quoting H. G. Wells,

> There was the cold and calculated scheme of the free and ambitious Latin Church to subdue and replace the emperor-ruled Byzantine Church; there was the free-booting instinct of the Normans . . . which turned readily to a new and richer world of plunder; . . . the intolerant Seljuks and the intolerant Fatimites lay now an impassable barrier across the eastward trade of Genoa and Venice that had hitherto flowed from Bagdad and Aleppo, or through Egypt. They must force open these closed channels, unless Constantinople and the Black Sea route were to monopolize Eastern trade altogether. . . .

While the forces that determined the Crusades were not put into motion by the desire to develop biology, the incidental biological results were notable. Contacts between the West and the East were reestablished; the biological works of Hippocrates, Aristotle, and Galen, in the original Greek, preserved and annotated by the Moslems, were introduced to Europe; new observations and comparisons of Asiatic and European organisms were made. Here were multitudes of new facts of natural history bearing on the yet undeveloped sciences of comparative anatomy and distribution; here was a fresh ferment that began the disintegration of the old biology and the formation of the new.

These activities in the growth of modern biology were greatly extended and accelerated by the later explorations of the 15th and 16th centuries. The primary aim of these voyages was the discovery of a new trade route to the Far East. While the achievement of such a route (Vasco da Gama's voyage, 1497–9 and Magellan's voyage, 1519–22) was of minor importance for trade, the discovery of America and its rich deposits of precious metals and other natural resources held by relatively undeveloped and practically defenseless natives, was highly significant; the desire to penetrate and exploit the new lands became a powerful impetus to a new wave of commercial expansion.

Incidentally biology was enriched. Says Miall, ". . . the discovery of a new world across the Atlantic immediately created a thirst for selfish acquisition accompanied by a far weaker but nevertheless invaluable impulse to learn all that could be learned about the new strange lands." This impulse to learn was, for biologists, particularly whetted by the reports of the entirely novel inhabitants of the Americas, and of animals and plants not listed or described by ancient or contemporary naturalists. This was especially true for the organisms of the West Indies, Brazil and Mexico, all explored by the Spanish; for, while North American species, as we now know, belong to genera or families which occur in Europe or temperate Asia, most of Central and South America comprise a far more peculiar flora and fauna.

Columbus on his first return in 1493 exhibited at the Spanish court Indians and parrots, and it was not long before Europe for the first time began to hear of the sloth, opposum, tapir, ant-eater, manatee, and humming-bird; the puma, llama, and alpaca; maize, cassava, pineapple, and prickly pear. Books describing the new natural history soon began to appear (See Miall, 1912). Peter Martyr Anglerius, chief chronicler of Indian affairs to Ferdinand and Isabella, published his *Decades* which was the chief source of information on the Americas to Europeans during the early 16th century. A crude attempt at, and what is more the need for, a science of comparative anatomy, is evident in his quaint description of the opposum, the chief oddity of the new fauna. He states it to have the snout of a fox, the tail of a monkey, the ears of a bat, the hands of a man, and the feet of an ape! A more definite biology of the new lands is to be found in the *General and Natural History of the Indies* (1526–1535) by Gonzalo Fernandez de Oviedo y Valdes (1478–1557), a mining inspector in America.

The effect of the new knowledge on the validity of the Biblical account of creation is thoroughly discussed in the *Natural and Moral History of the Indies* (1588) by the Jesuit Father Joseph de Acosta (1540 ?–1600) who did service in Peru. Acosta, it is true, adheres to the story of the *Book of Genesis*, but his work shows an awareness of the new problems of distribution raised by the discovery of America and attempts their solution by the hypothesis that New World organisms migrated from the Old World by way of a land bridge between the continents somewhere near the North Pole. Be that as it may, Acosta's reasoning vividly illustrates how the exploration of the New World led to results that weakened belief in the Biblical account of special creation by forcing the validity of this doctrine to depend on the assumption of adventitious hypotheses.

Contemporaneously with the opening up of the Americas, somewhat similar explorations were bringing Europeans into contact with the African coastline and tropical Asia. Colonization in these lands, however, was much more difficult than in the New World, due to the extreme distances of travel, the excessive heat of the East Indies, and the resistance of the more populous and better armed natives. Hence, it was not until much later—the 17th and 18th centuries—that Europeans successfully penetrated some parts of the East. At this time, when colonization had become profitable, the Dutch began to publish methodical treatises of the natural history of India and the Malay Archipelago.

The period from the late 15th to the 18th century thus was a time of widespread exploration primarily concerned with political and economic expansion. The discovery and colonization of the new lands led to quite unanticipated biological results—the accumulation in Europe of vast numbers of new facts of natural history. And it is significant for our thesis to note that this accumulation was, at first, not effected by biologists, but by those directly concerned with the more fundamental social aspects of the conquest of the new lands; by Columbus, the explorer; Anglerius, the court chronicler; Oviedo, the mining inspector; and Acosta, the missionary. Nevertheless, a new biological world had been opened—more extensive than the old and often at variance with it.

It was long before the new material was comprehensively appreciated. Says Miall,

> The scientific gain which accrued from the multitude of new species was not really so great as it appeared to be. So vast and sudden an accession of

facts overpowered rather than strengthened the infant studies of zoologists and botanists. . . . Europe was as ill-prepared to grasp the new opportunities of enlarging the knowledge of terrestrial life as politically and morally ill-prepared to use her conquests in Mexico and Peru to the lasting advantage of mankind. . . . It was not till the Age of Buffon that comprehensive and daring questions were raised in earnest, and that the new sciences of geology and paleontology began to enforce the pregnant thought that facts unintelligible on the theory of sudden creation might receive an explanation from long-continued development. . . .

And, as we shall soon see, this contributing rôle of geology and paleontology in the coming of evolution, was itself determined by underlying social factors.

The 19th century witnessed a new period of exploration related to the consolidation of the earlier conquests into the colonial empires of contemporary European nations. These voyages also led to an incidental biological counterpart which, in reflection, as it were, of its politico-economic mediation, was chiefly concerned with the consolidation of its facts into an integrated theoretical system.

Among the 19th century explorations, the voyage of H.M.S. *Beagle* (1831–1835) carrying the young Charles Darwin as naturalist was for the history of biology an obviously outstanding event. It is not uncommon to find that present-day biologists are of the opinion that the voyage of the *Beagle* was primarily a biological expedition. This view is erroneous. Darwin himself states in the opening paragraph of his book *The Voyage of the Beagle* that,

The object of the expedition was to complete the survey of Patagonia and Tierra del Fuego, commenced under Captain King in 1826 to 1830—to survey the shores of Chile, Peru, and of some islands in the Pacific—and to carry a chain of chronometrical measurements around the world.

The position of Darwin as a dependent supernumerary on board the *Beagle* is clearly expressed in the following quotation from the preface of the same work,

. . . it was in consequence of a wish expressed by Captain Fitz Roy, of having some scientific person on board, *accompanied by an offer from him of giving up part of his own accommodations*, that I volunteered my services which received, through the kindness of the hydrographer, Captain Beaufort, the sanction of the Lords of the Admiralty. *As I feel that the opportunities which I enjoyed of studying the Natural History of the different countries we visited, have been wholly due to Captain Fitz Roy, I hope I may here be permitted to repeat my expression of gratitude to him;* and to add that, during the five years we were together, I received from him the most cordial friendship and steady assistance. (My italics; A. S.)

It is therefore indubitable, to quote Henshaw Ward, that "the work with time and soundings was the chief mission of the *Beagle*; collecting specimens by a naturalist was only a supplementary job."

Nor was this relation between the Beagle and Darwin an isolated instance. The voyage of the Beagle is only one of a series initiated by the British Government for the purpose of making world-wide surveys touching on the possibilities of existence of exploitable natural resources and concerned with the improvement of trade routes, particularly among the far-flung British colonies. As a rule, biological research was not specifically included in the program of activities. But biologists were quick to seize the opportunities offered by these trips to study organisms in their native habitats all over the world. Thus, Thomas Huxley on board H.M.S. *Rattlesnake* and Joseph Dalton Hooker on board H.M.S. *Erebus* were enabled to investigate problems in exotic natural history under circumstances quite similar to Darwin's.

There is no need to go into detail concerning the biological consequences of these explorations. *The Voyage of the Beagle* which exemplifies them has long been a classic in the literature of natural

history; its observations and the rôle that they played in leading the thought of Charles Darwin into evolutionary channels are well known (see Ward). Darwin, himself tells us in his autobiography that, "The Voyage has been the most important event in my whole life. . . . I owe to the voyage the first real training or education of my mind." Certainly it is clear that after Darwin returned to England—his mind packed with observations of the multiplicity and variability of species, of peculiarities of distribution (e.g. in the Galapagos), of details of geology and paleontology, and because of all of these observations believing no longer in special creation—he almost immediately began research in the "mystery of mysteries"—the origin of species. The voyage of H.M.S. *Beagle*, like the Crusades and like the expeditions captained by the Columbuses and the Magellans, while prompted not by biological, but by broad economic forces, nevertheless yielded biological results of decisive importance in the coming of evolution.

III

It has already been stated that social forces were involved in the growth of geology and paleontology. These sister sciences arose in close relation to the development of the mining industry. Let us examine this relation.

There had been geological and paleontological observations as far back as classic Greek times, but they were scattered and unintegrated. Up to even as late as the early 18th century such fossils as had been unearthed were considered to be *lusus naturae*—jokes of nature. But with the development of mining in early modern times fossils in such great numbers were dug up with ores and precious minerals that this view could no longer be maintained. Thus the foundation of fact for the science of paleontology was laid.

Previous to the 18th century there were no sciences of geology and paleontology. In fact, the word fossil during these times did not even have its present meaning. According to Arber, fossil originally meant anything dug up out of the earth, e.g. sands, clays, marbles. During the scientific renaissance a distinction was made between "real or natural" fossils—lithological, petrological, or mineralogical specimens—and "extraneous" fossils, the "fossils" of today. The very terms used emphasize the primacy of mining over paleontology. An exactly similar relation holds in the geological sphere. Wolf states that, "The terms 'geology' and 'geologist' did not come into general use till late in the 18th century, when they gradually displaced the older terms 'mineralogy' and 'mineralogist' . . ." The chief work of this period of interest to the modern geologist is the *De Re Metallica* (1556) by Georgius Agricola (1494–1555)—a classic in the technology of mining engineering (see Wolf). While this work primarily deals with the problems of practical mining, such as surveying, engineering, smelting, assaying, the qualities of competent miners, business administration, there is some discussion of more purely geological matters like stratigraphy, the effects of water and wind, and the origin of ore deposits.

Mining in the 16th century was essentially a surface affair, for the depth to which shafts could be sunk was sharply limited by the capacity of the mechanical contrivances then used to pump water out of the mines. The invention of the steam engine (1765), which played such a dominant rôle in bringing about the Industrial Revolution, particularly revolutionized mining. The new source of power was, in fact, first used to pump water out of

mines; hence its early name, "The Water Commanding Engine" (see Wolf). It was now possible to sink shafts to much greater depths, the mining industry grew apace; and these new conditions were almost immediately reflected in a rapid rate of integrated growth in both paleontology and geology. The practical impetus back of this growth is indicated in the occupations of some of the important geologists of this period: Abraham Gottlob Werner (1750–1817) was professor at the mining academy at Freiberg and, quoting Nordenskiöld, "systematically explored the geology of his own district . . . and on the results thereof based a rational mining-industry . . ."; James Hutton (1726–1797), who published his *Theory of the Earth* in 1785, was a farmer; William Smith (1769–1839), was "a surveyor and leveler" (Nordenskiöld); and Christian Leopold von Buch (1774–1853), was a student under Werner.

The more the mining industry grew, and the more geological problems were investigated, the greater were the number of fossils dug up and studied. The dependence of the development of paleobotany on the mining of coal, for example, is plain to see in the contents, and, for that matter, in the very title, of Arber's paper. The reference to the "British coal measures" is essentially a reference to the British coal mines, and it was the working of these mines that unearthed the fossil plants. As for the growth of paleozoology, Cuvier himself was dependent on a type of mining, as the following quotation from Nordenskiöld shows:

> . . . he soon found himself engaged in a special field of research, the study of fossil forms. As is well known, Paris is situated in the center of a calcareous district in which the stone used for building-material is particularly rich in fossils, . . . it was on the basis of material gathered from this and other districts that he formed his theory of the evolution of the earth and of the creatures living on it.

Mining, and its related industry, quarrying, were thus instrumental in the development of geology and paleontology. These economic pursuits, like exploration, acted as fact-collectors, literally unearthing the facts, and, what is more, as they grew, were faced with practical problems concerning the structure and manipulation of the earth's crust, the solutions of which helped to establish the science of geology. It is a far cry from these rather elementary conditions attending the birth of geology to the grand sweep of Lyell's *Principles of Geology* (1831), but an unbroken development leads from the former to the latter. Lyell's work is of particular interest because it was one of the few books that Charles Darwin studied while on H.M.S. *Beagle*, and it was in this volume that he learned of the principle of uniformitarianism, a doctrine that was destined to play an important part in his investigation of the problem of evolution.

IV

When Darwin returned from the voyage of the *Beagle* in 1835 he was quite convinced that species could not have originated by special creation. Biology had developed to such a state at this time that it was possible, from the facts of natural history, distribution, comparative anatomy, and paleontology, to infer an alternative method—evolution. This, of course, is well known; it has been our interest, however, to demonstrate the rôle of non-biological social processes in helping to establish these branches of biological learning.

Darwin was not content with merely inferring evolution. He hoped to show by observations on living organisms that the species-type was not fixed, that it might suffer change; i.e. he wished to demonstrate evolution in miniature. Furthermore, he longed to discover the

mechanism of evolution. The first problem was successfully investigated by a series of researches on variation and selection of domesticated plants and animals begun in 1837. The second problem was solved by assuming the mechanism of natural selection after reading Malthus in 1838. From these dates until 1859 Darwin painstakingly worked on these two problems, combining the results of his research with knowledge derived from other fields, slowly perfecting his conception of evolution. We will now present the social factors in these two main lines of his work, treating them separately, since they present rather separate social origins.

The research in variation was modelled along the lines of Lyell's principle of uniformitarianism. The general method and results of the problem are clearly stated in Darwin's autobiography:

> After my return to England it appeared to me that by following the example of Lyell in geology, and by collecting all facts which bore in any way on the variation of animals and plants under domestication and nature, some light might perhaps be thrown on the whole subject (of origin of species). My first notebook was opened in July 1837. I worked on true Baconian principles and without any theory collected facts on a wholesale scale, more especially with respect to domesticated productions, by printed enquiries, by conversations with skillful breeders and gardeners and by extensive reading. . . . I soon perceived that selection was the keynote of man's success in making useful races of animals and plants.

Darwin was able to pursue his program of research because of the high level of attainment reached by practical breeding in England in the middle of the 19th century. (See articles in the *Encyclopaedia Britannica*, 14th ed. New York, 1929; Animal Breeding; Robert Bakewell; and under Agricultural Articles—Live Stock and Other Improvements; Capitalist Farming and the Great Landlords.) The improvement of British live stock was begun about 1750. At this time there were no definite breeds; there was great variation among the many local types. Robert Bakewell (1725–1795) in 1760 began the systematic improvement of sheep, oxen, and horses and soon produced the famous breeds of Leicestershire sheep and Dishley cattle. So successful were his methods and so certain the results of breeding from his carefully selected stock, that it was possible for him to establish a trade in ram-letting that netted him a profit of over 1200 guineas in one year from one ram. Artificial selection was entering the arena of commerce.

The business of artificial selection started by Bakewell was extended by others, and to the further improvement not only of live stock, but of domesticated plants as well. The growing needs of the rising English bourgeoisie had created a market for better beef, milk, wool, pork, draught animals, and plant products; and the plant and animal husbandmen, untroubled by any qualms concerning the sanctified fixity of species, but with good business acumen, took advantage of the plasticity of species so that they could improve their commodities and profit from the rising market. In the activities of the artificial breeders Darwin was quick to discern the essential biological basis—the mutability of species. Hence his interest in their business; for if it were accepted that species-types could be changed by man through artificial selection, and the laws of these changes could be discovered, it was but a step following Lyell's uniformitarianism, to apply these findings to the process of evolution in nature—past as well as present.

Darwin not only "collected facts . . . with respect to domesticated productions," and had "conversations with skillful breeders and gardeners," but he became a member of several breeding

clubs—an unheard of association: the scholar, in his search for pure knowledge, mixing with the business-man breeder and fancier—and, moreover, he carried on experiments in selection with ornamental poultry, such as fancy pigeons. His colleagues, who evidently considered Darwin's *metier* to lie in geology, natural history, and taxonomy, as exemplified by his then current work on the structure and classification of the barnacles, must have regarded these breeding excursions with some misgivings. Their attitude is amusingly reflected in the following quotation from a letter of Darwin to Hooker:

> In your letter you wonder what 'ornamental poultry' has to do with Barnacles, but do not flatter yourself that I shall not yet live to finish Barnacles, and then make a fool of myself on the subject of species, under which head ornamental poultry are very interesting.

The results of Darwin's labor on the subject of variation and selection were in part incorporated into his *Origin of Species*, but the complete report did not appear until some ten years after this (1868) when he published his *Variations of Animals and Plants under Domestication*. Our consideration of the manner in which Darwin gathered materials for this work leaves no doubt that it is rooted in the practical activities of plant and animal breeders—activities motivated, not by the desire for knowledge, but for profit. The utilitarian character of this work is incidentally indicated by the titles of books in the advertisements that grace the fly-leaves of the American edition of *Variations* such as: 'The Miniature Fruit Garden,' 'Practical Horticulturist,' 'Draining for Profit,' 'Gardening for Profit.' . . The aura of lucre that enveloped his investigations is well illustrated in Darwin's clinching his argument proving the "force of inheritance" (Chap. XII of *Variations*):

> Some writers (Buckle in History of Civilization; Mr. Bowen, Professor of Moral Philosophy in Proc. American Acad. of Sc. V, p. 102) who have not attended to natural history, have attempted to show that the force of inheritance has been much exaggerated. The breeders of animals would smile at such simplicity; and if they condescended to make any answer might ask: what would be the chance of winning a prize if two inferior animals were mated together? . . . Why have pedigrees been scrupulously kept and published of Shorthorn cattle and more recently of the Hereford breed? . . . Have the Shorthorns, without good reason, been purchased at immense prices, and exported to almost every corner of the globe, a thousand guineas having been given for a bull? . . . With pigs, the Yorkshire and Cumberland breeders 'preserve and print pedigrees' and to show how such highly bred animals are valued, I may mention that Mr. Brown who won all the 1st prizes for small breeds at Birmingham in 1850 sold a young sow and boar of his breed to Lord Ducie for 43 guineas; the sow alone was afterwards sold to Rev. F. Thursby for 65 guineas who writes, 'She paid me very well, having sold her produce for 300 l, and having now four breeding sows from her.' *Hard cash paid down over and over again is an excellent test of inherited superiority*. . . . (My italics: A.S.)

We may well imagine the Reverend preaching of a world of fixity of species on Sundays, but on week-days profiting in "hard cash paid down over and over again" from the changeability of species!

These profit-making origins of Darwin's research, however, did not prevent him from extracting the biological essence, for, in conclusion, he states: "In these laws of inheritance as displayed under domestication we see an ample provision for the production through variability and natural selection of new specific forms."

V

In his autobiography Darwin states, after admitting his indebtedness to "man's success in making useful races of animals and plants": "But how selection could be applied to organisms living in a state of nature remained for some time a mystery to me."

How the mystery was cleared up is given

in the next paragraph of the autobiography:

> In October 1838, i.e., 15 months after I had begun my systematic enquiry I happened to read for amusement 'Malthus on Population'—and being well prepared to appreciate the struggle that everywhere goes on from long continued observation of the habits of plants and animals, it at once struck me that under these circumstances favorable variations would tend to be preserved and unfavorable ones be destroyed. Here then I had at last got a theory by which to work.

The dependence on Malthus may seem to be quite fortuitous. This view, however, is difficult to maintain in view of the fact that Alfred Russel Wallace in 1858 independently propounded natural selection as the mechanism of evolution from a sudden realization of the biological significance of Malthus' work. Wallace in the "Introductory Note" to the second chapter of his 1895 edition of *Natural Selection and Tropical Nature* tells how, in February 1858, he

> . . . was suffering from a rather severe attack of intermittent fever at Ternate in the Moluccas . . . and something led me to think of the positive checks described by Malthus in his 'Essay on Population,' a work I had read several years before, and which had made a deep and permanent impression on my mind. These checks—war, disease, famine, and the like—must, it occurred to me, act on animals as well as on man. Then I thought of the enormously rapid multiplication of animals, causing these checks to be much more effective in them than in the case of man; and while pondering vaguely on this fact there suddenly flashed upon me the *idea* of the survival of the fittest—that the individuals removed by these checks must be on the whole inferior to those that survived. In the two hours that elapsed before my ague fit was over I had thought out almost the whole of the theory, and the same evening I sketched the draft of my paper, and in the two succeeding evenings wrote it out in full, and sent it by the next post to Mr. Darwin.

Darwin's consternation upon receipt of this paper which almost exactly duplicated the yet unpublished results of his twenty years' research on natural selection, and the way in which the suddenly arisen problem of priority was handled by Lyell and Hooker in consultation with Darwin will always remain one of the most dramatic episodes in the history of science. With both Darwin and Wallace, however, admitting their indebtedness to the *Essay on Population*, the conclusion seems inescapable that Malthusianism is directly responsible for the genesis of the theory of natural selection.

To comprehend the social factors of interest here we must trace the origin of Malthus' work. The *Essay on Population* first appeared in 1798 as part of a polemic on the perfectibility of man in which Condorcet, the French metaphysician, and Godwin, the English author and politician, and Malthus, were the principal participants. (Note the extended title of Malthus' book as given in the bibliography.) The social conditions in the last decades of the 18th century that provoked the controversy were in England precipitated by the Industrial Revolution and are well summarized by Wells in *The Outline of History:*

> . . . . The immediate effect of the Industrial Revolution upon the countries to which it came, was to cause a vast, distressful shifting and stirring of the mute, uneducated, leaderless, and now more and more propertyless common population. The small cultivators and peasants, ruined and dislodged by the Enclosure Acts, drifted toward the new manufacturing regions, and there they joined the families of the impoverished and degraded craftsmen in the factories. Great towns of squalid houses came into existence. Nobody seems to have noted clearly what was going on at the time. It is the keynote of 'private enterprise' to mind one's own business, secure the utmost profit and disregard any other consequences. Ugly factories grew up, built as cheaply as possible, to hold as many machines and workers as possible. Around them gathered the streets of workers' homes, built at the cheapest rate, without space, without privacy, barely decent, and let at the utmost rent that could be exacted. These new industrial centres were at first without schools, without churches. . . .

France, for different reasons, had been plunged in revolution during this period.

But there was unrest and the thread of revolt in England too. For example, in 1795, says Bonar: "there had been a serious scarcity; war prices had become famine prices. It was the year when 'the lower orders' were held down by special coercion acts; it was the year when the king's carriage was stopped by a mob crying 'Bread, bread!' "

Whatever the details, it is evident that human society in a good part of Europe in the 1790's was in the grip of a bitter struggle for existence. There was hope in the minds of such utopians as Condorcet and Godwin that the condition of man could be perfected. Malthus, on the contrary, could see only a gloomy future, for he believed that human society was suffering from the inevitable effects of the operation of a natural law,—the tendency of population to increase faster than the means of subsistence—and that as a result of this the poor and the inept, those unable "to get on" in society, must be thinned out by the ruthless agencies of poverty and hunger, vice and crime, pestilence and famine, revolution and war.

It is not our concern to attempt an evaluation of the various solutions offered by the controversialists in this polemic, nor to trace the subsequent breakdown of Malthusianism as a theory of human population. It is sufficient for us to point out that a human struggle for existence, and a theory of the survival of the fittest, are inherent in the Malthusian interpretation of human society, and that the mechanism of natural selection was elaborated in analogy to these supposed conditions of human social existence.

VI

It is now clear that the development of Darwinism was importantly conditioned by a number of precedent non-biological social factors. The Crusades, the exploration and conquest of newly discovered lands, the growth of mining, the business of practical breeding, the complex Malthusian social background, all contributed in one way or another to the elaboration of Darwin's research. Within the short space of this article it has not been possible to trace in full detail the manner in which each of these social factors affected their corresponding biological fields. For example, the relation between exploration and the growth of comparative anatomy has been merely indicated, and it has not been possible to discuss the social conditioning of the growth and failure of Lamarckism—problems which we intend treating in later work.

In considering the relative directness with which the various social conditions affected Darwin in his investigations of the problem of the origin of species it is evident that breeding and Malthusianism take first place. For Darwin himself mingled with the plant and animal breeders; he read their journals; he studied their business at first hand. The relation to Malthusianism is much less extensive but none the less direct. Moreover, the results of these social contacts were of prime importance in scientifically establishing the truth of evolution. For in the results of his examination of artificial selection Darwin found indubitable proof of the plasticity of species; like Lyell in his study of currently operating geological forces, he discovered that the investigation of organic transformations led to conclusions that could be applied to evolution in general. And the Malthusian doctrine of a struggle for existence and the survival of the fittest arising from the contradiction between great reproductive tendencies and relatively little means of subsistence was taken over bodily to form the basis of the theory of natural selection. Although only a coincidence

may be involved, it is interesting to note that artificial selection and natural selection, so straightforwardly related to social conditions, were the biological supports upon which Darwin in *The Origin of Species* leaned heaviest in setting up his proof of evolution.

While the individual social factors we have discussed differ in respect to their weight in influencing the rise of evolution they have in common one very significant basic element: they are all, in essence, economic; they all involve the production and exchange of saleable goods—commodities—or the economic consequences of these activities. Mining, and artificial selection also, as Darwin was so keenly aware, are obviously concerned with the production of commodities; the Crusades, as the excerpt from Wells demonstrates, were strongly motivated by a complex of economic causes; the 15th and 16th century explorations were initially undertaken to improve the exchange of commodities between Spain and Portugal and the Far East, and the unexpected discovery of America, again quoting Miall, "created a thirst for selfish acquisition"; the expeditions of the early 19th century, while carrying on physiographical and chronometric investigations of a high scientific order, were primarily determined by the economic expansion connected with the growth of the present European empires, and, finally, Malthusianism was a direct outgrowth of the social conditions resulting from the Industrial Revolution.

Furthermore, all these economic activities were not unrelated phenomena; they represent various phases of one broad historical movement—the evolution of feudal into capitalist society. Indeed, the development of the very social factors we have been discussing in relation to the genesis of Darwinism in great measure constituted the complex historical flux out of which capitalism crystallized. The earliest emergence of the new society occurred in England (in the very late 18th century), for, in comparison to the rest of Europe, it was this country that manifested the most intensive development of the social activities underlying the change to capitalism. It is this that accounts for the fact that English thinkers of the early 19th century—Spencer, Lyell, Wallace, not to mention Darwin,—played such a predominant and decisive rôle in the coming of evolution. Darwinism sprung up where and when capitalism was most strongly established.

While the economic activities of developing capitalism clearly played a fundamental rôle in the genesis of evolution, the question might still be asked as to whether they were necessary. That is to say—could Darwinism have been produced in the absence of the social factors we have discussed. This, of course, cannot be answered. It is impossible for us to go back and rearrange the course of history in order to see what would have taken place under some different set of precedent conditions. All we can do is to point to the historical development as it—in fact—occurred.

At the end of the beautifully appreciative eulogy, *The Debt of Science to Darwin*, Wallace, paraphrasing the famous couplet of Pope, writes:

"Nature and Nature's laws lay hid in night;
God said, 'Let Darwin be,' and all was light."

This is a just tribute to Darwin, and nothing can ever detract from the greatness of the man and his work. In fact it is to his greater glory that he found pabulum for development not only in the purely biological, but also in the basic social phenomena of his age.

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MERRIAM'S LIFE ZONES OF NORTH AMERICA

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OUR present concepts of bioclimatic provinces have had their origin in purely floristic and faunistic studies. With the development of the taxonomy of North American species various distributional patterns appeared, and whenever several of these seemed related they were made the basis of a province. This aspect of biogeography was popular during most of the past century. By 1889, when the Division of Ornithology and Mammalogy of the United States Department of Agriculture undertook its important work in this field, over 50 papers had appeared proposing biotic provinces for North America, about half of them based on floristics and the others on faunistics. Although certain of these pioneer classifications are difficult to coördinate with the others, there was sufficient agreement that in summarizing these papers Merriam (1892) found that North America had been divided into distributional provinces as follows: Arctic; Boreal (subdivided by Allen into Hudsonian and Canadian); Eastern or Atlantic (variously subdivided into Carolinian, Alleghanian, Southern Florida, Austroriparian, Louisianan, etc.); Central; Western or Pacific; and Sonoran (including the closely related Baja California).

Life zone studies by the Division of Ornithology and Mammalogy were begun in the summer of 1889. This venture, suggested by Clinton Hart Merriam, who was then chief of the Division, was prompted largely by economic motives. It was held that a knowledge of life zone boundaries would provide a key to agricultural possibilities which otherwise would have to be determined by the trial and error method for every location.

The first field work consisted of a biological survey of the San Francisco Peak area in Arizona (Merriam, 1890). As a result of observations and collections of the biota with particular emphasis on the altitudinal limits of the species, Merriam, who personally directed the field work, recognized seven major biotic zones: Alpine, Subalpine or Timberline, Hudsonian or Spruce, Canadian or Balsam fir, Neutral or Pine, Piñon, and Desert. Each of these zones was characterized by a group of species not found in the others. Those zones in which the characteristic species were boreal in affinity were grouped into a Boreal Division (including the first 4 zones above), and where the indicators were of southern extraction (the last 3 above) the term Sonoran Division was applied. The fifth zone was called "Neutral" (later referred to as "Transition") because it contained elements of both boreal and austral derivation; however, it was finally classed as Sonoran because of the preponderance of the latter group.

Since certain species of the San Francisco Mountain zones also occurred in the zones previously described for North America, Merriam tried to coördinate his regions with those outlined above. Only his Boreal Division seemed to fit in well with the previous classifications. The integration of these new western findings with

the earlier studies of others was expressed in a map which is remarkably similar to the map of climax plant formations by Weaver and Clements (1929). It is to be noted that this first map, which was probably his best, was based entirely on data of biogeographical distribution.

In this first paper, little attention was given to the causes of the zones. Some of the statements in this regard, however, are significant in the light of future developments. Without indicating the sources of information, Merriam empirically stated that "temperature and humidity are the most important causes governing distribution, and that temperature is more potent than humidity" (*loc. cit.*, p. 26). Previous workers had held that, for birds, the temperatures of the breeding season are more critical than at any other times of the year, so Merriam asks "if this is true of birds, why is it not true of other forms of animal life and of plants as well?" (*loc. cit.*, p. 26). Winter temperatures were considered of little significance since animals either migrate or hibernate or are very hardy, and "freezing does not hurt most plants when not in a state of reproductive activity" (*loc. cit.*, p. 27). It is remarkable to note the diligence with which meager temperature records for various altitudinal zones were compiled, and yet in the discussion of precipitation, no quantitative data were presented, and the altitudinal variation of this factor was ignored.

In the course of subsequent field work (Merriam, 1891, 1893) the preliminary system was modified and expanded to include all of North America, and in 1898 the classification in its final form was published (Merriam, 1898). By this time, many data had been accumulated on the varieties of agricultural plants which could be grown successfully in each zone. A classification of these plants, together with a brief summary of the temperature data on which the explanations of the zones were based, was given in this paper.

The geographic provinces and their approximate distribution were listed in order by Merriam as follows.

The *Boreal Region* included all of the continent from the polar seas southward to the northern boundary of the United States of America, and extended southward along the principal mountain systems. It was subdivided into three zones. The *Arctic-Alpine Zone* was the area above the latitudinal and altitudinal limits of tree growth. The *Hudsonian Zone* took in the northern part of the Canadian forested area, and similar forest zones just below timberline on the mountains farther south. The *Canadian Zone* designated the southern forest region of Canada including parts of the northern tier of the eastern United States, and a belt below the Hudsonian Zone in the mountains.

An *Austrial Region* lying at lower latitudes and altitudes than the preceding was also divided into three zones, each of which in this case was further subdivided into two or three Faunal Areas. These zones were again essentially latitudinal belts distinguished climatically only by temperature, but the subdivisions (the Faunal Areas) were based mainly on humidity. *The Transition Zone* was so named since it seemed to be a region where the ranges of many boreal and austral elements overlapped. It included the Alleghanian Faunal Area (New England to North Dakota, and down the mountains to Georgia), the Arid Transition Faunal Area (the Dakotas, eastern Montana, Manitoba, Alberta, and around the bases of the Western mountains), and the Pacific Coast Transition Faunal Area (the hygrophytic forest region of Washington, Oregon, and California). The *Upper Austral Zone* was divided into a humid eastern

Carolinian Faunal Area (South Dakota to Oklahoma and eastward to Ohio and South Carolina), and an arid western Upper Sonoran Faunal Area (at low elevations west of the 100th meridian). A *Lower Austral Zone* consisted of a Lower Sonoral Faunal Area (west of the 100th meridian, from Texas to the Sacramento Valley and southward into Mexico, taking in the major deserts of North America excepting the Great Basin), an Austroriparian Faunal Area (from eastern Texas and Oklahoma around the Atlantic coastal plain to Virginia), and a Semitropical (or Gulf Strip) Faunal Area (a very narrow coastal strip from southern Texas to Florida, including all of the latter but the southern tip).

The *Tropical Region* extended northward along both coasts of Mexico projecting into the United States of America in the Colorado River Valley and southern Texas. In the east it included the southern third of peninsular Florida.

By way of explaining the distribution of these provinces Merriam stated (*loc. cit.*, p. 54) that "apart from obvious mechanical barriers such as oceans, temperature is the most important single factor in fixing the limits beyond which particular species of animals and plants cannot go. Investigations conducted by the Biological Survey have shown that the northward distribution of terrestrial animals and plants is governed by the sum of positive" mean daily "temperatures," using 6°C as the threshold value, "for the entire season of growth and reproduction, and that the southward distribution is governed by the mean temperature of a brief period" (using the mean normal temperature of the six hottest consecutive weeks) "during the hottest part of the year." He had previously stated (Merriam, 1894) that the southern limits (conditioned by the mean temperature of the hottest part of summer) of the Arctic-Alpine, Hudsonian, Canadian, Transition, and Upper Austral Zones coincide with the isotherms of 10°C, 14°C, 18°C, 22°C, and 26°C respectively. On the other hand, the northern limits of the Transition Zone, Upper Austral Zone, Lower Austral Zone, and Tropical Region correspond with summation isotherms of 5,500°C, 6,400°C, 10,000°C, and 14,500°C respectively. These are all the data ever presented by Merriam in this connection. As shown on maps (*loc. cit.*) the two sets of isotherms practically coincide with each other and with his life zones.

Merriam's work was readily accepted by many biologists. It was indeed a great step forward to utilize both floristics and faunistics as bases for life zones, and at the same time to offer at least a plausible climatic explanation of the provinces. The great accuracy of description of the zones in the western states, and the prestige which the government rendered by sponsoring the investigation as well as by using the system also helped to bring about an immediate acceptance of the classification. Checking up on the explanations of the zones would have been a laborious task. Moreover, it probably seemed futile since Merriam had already availed himself of all the Weather Bureau records. The fact that scarcely any of the fundamental computations were ever published was another impediment. It was easiest to assume that the explanations were sound, and for a time they were never questioned.

In 1902 Adams cast considerable doubt on the concept that distributional patterns conform to latitudinal zones. His maps of species ranges showed them to be related to each other in groups, each of which has a common center of distribution. A few years later Transeau strengthened this latter concept by demonstrating that

centers of distribution are correlated with variation in the precipitation/evaporation ratio. In this paper he expressed his disbelief that distribution could be explained by the temperature factor alone, and suggested the precipitation/evaporation ratio as a method of integrating the most critical factors of plant environment.

During the next two decades the researches of other botanists consistently refuted Merriam's temperature "laws." After an exhaustive study of correlations between the facts of plant distribution and all phases of available climatic data, Livingston and Shreve (1921, p. 528) stated that "the system of life zones worked out by Merriam . . . will require much modification before it may become at all satisfactory to a serious student of etiological plant geography," and eleven years later two lists of criticisms were compiled, this time by zoologists (Kendeigh, 1932; Shelford, 1932). In the following paragraphs is an attempt to summarize the criticisms from all sources.

Destructive criticisms of Merriam's work fall into three categories, viz.: (1) the descriptions of the zones, (2) the explanations of the causal factors, and (3) technical errors.

(1) One of the most significant findings of our active investigations of successional phenomena in the past three decades has been the discovery that the nature of those climax vegetation types which are determined by climate is more significant as an indicator of the totality of climatic influence than are any or all of the climatic instruments yet devised by man. Ecologists now recognize a grassland climate, a summer-green forest climate, a winter-green (monsoon) forest climate, a broad sclerophyll scrub (Mediterranean) climate, etc. If we compare the central grassland province of North America (as mapped by Weaver and Clements, 1929) with Merriam's map, we are immediately struck with the fact that this biotic entity is severed by Merriam into three parts, each of which is linked up by him with one or more distinctly different types of climatic climax vegetation (Transeau, 1905). For example, one of these three segments is included in the Transition Zone along with deciduous forest, ponderosa pine forest, sagebrush, broad sclerophyll scrub, and redwood forest—all climatic climax types. If we follow Merriam in this, we must assume a closer relationship of this grassland segment to these other vegetation types than to the remainder of the grassland! As mentioned above, Merriam's first map of biotic provinces (Merriam, 1890) is in remarkably close agreement with the known distribution of climatic climax vegetation, but the final map was so greatly influenced by instrumental data that it is essentially a climatic map. Thus as a result of using the floristic and faunistic viewpoint (rather than the dynamic sociological one which had not yet been expounded) and emphasizing instrumental data, certain natural entities were artificially split, while very diverse vegetation types were at times lumped together.

Although the zonal descriptions for the mountains where Merriam worked are very good, the lumpings and correlations from region to region are highly objectionable, especially in his Austral Region.

(2) Neither field nor laboratory tests were made to substantiate the temperature hypotheses used as bases for the explanation of distribution. Inferences drawn from very meager studies of a few organisms were assumed to hold true for all forms of life. The same threshold value (6°C) was used for all species of plants and animals. This is obviously invalid when one considers tropic and arctic species, even if older experiments (DeCan-

dolle, as quoted by Kendeigh, 1932) had not already proved it so. The minimum temperature for growth is as low as −2°C for the garden pea, *Pisum sativum*, (Leitch, 1916) and as high as 15–18° for melon and cucumber (Maximov, 1930).

The method of summation used by Merriam is theoretically unsound since each degree of temperature is assumed to have the same significance, and it is now well established that chemical reactions in plants tend to follow Van t'Hoff's Law until temperature high enough to injure the plant is attained (Livingston and Livingston, 1913; Lehenbauer, 1914; Livingston, 1916; Maximov, 1930).

The few detailed studies of temperature summation which have been made in recent years indicate that this idea is of no great significance in connection with plant growth (Seeley, 1917).

It has been pointed out that temperature extremes are much more important for animals than are mean figures for a certain span of summer (Kendeigh, 1932).

Winter temperatures are certainly not to be so completely ignored. Merriam (1894) asserted that low temperatures were significant only in the case of certain plants sensitive to killing frosts. The variation in the ability of plant species to withstand low temperatures is undoubtedly a significant factor in plant geography. Minimal temperatures of winter have been found to have a direct bearing on plant distribution (Shreve, 1911 and 1914; Salmon, 1917; Uphof, 1920; Mason, 1925).

It has been noted that most temperature data give isotherms which roughly parallel latitudinal or altitudinal lines, so that by picking the proper range from any type of temperature data one may get correlation with altitudinal or latitudinal biotic zones (Kendeigh, 1932).

All of these errors have come about through trying to explain the facts of distribution on a single factor of the environment (Transeau, 1905), whereas we now hold the environment to be such an intricate complex of interdependent factors that it is exceedingly difficult, if indeed not an impossibility, to attempt to evaluate the individual influences.

(3) Several important technical errors were made which render the work less scientifically sound. As mentioned above, only the final generalized temperature figures were ever given to substantiate this division of an entire continent into climatic zones. The individual summations on which the isotherms were based were always omitted from the maps. A climax to this obscure procedure came when Merriam himself admitted (1899) that through a misunderstanding with the Weather Bureau, 0°C had been used as a threshold value in the summations rather than 6°C! The agreement of zones with such faulty isotherms is proof of the invalidity of his summation "law" (Kendeigh, 1932).

Finally, it is not the way of the true scientist to subscribe to the complacent attitude expressed in the following paragraph (Merriam, 1895): "It appears, therefore, that in its broader aspects the study of the geographic distribution of life in North America is completed. The primary regions and their subdivisions have been defined and mapped, the problems involved in the control of distribution have been solved, and the laws themselves have been formulated."

In all fairness, however, we must recognize the value of Merriam's life-zone work as a contribution to biology. His was the first major attempt to use climatic data in interpreting the distribution of North American biota, and to base distributional provinces on both plants and animals. It is also to Merriam's credit

that he deplored the use of daily, monthly, or annual climatic data and recommended that the intervals used have less arbitrary bases (Merriam, 1894). Although his explanations may be for the most part fallacious, they have greatly stimulated inquiry into the causes of plant and animal distribution.

The descriptive parts of his field surveys were very accurate and for a long time have been standard references on vegetation zones of western United States of America. Indeed many western biologists, chiefly zoologists, still use Merriam's terminology for the various zones although for the most part they have been replaced by more descriptive terms such as spruce-fir zone, sagebrush zone, etc. And while his map is not acceptable outside of the regions where actual field work was done, it has been useful as a guide which could be verified or disproved by biologists working in restricted portions of the continent.

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ESSAYS ON EVOLUTION

III. ON THE ORIGIN OF INTERSPECIFIC STERILITY

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THE problem of the origin of interspecific sterility has long presented the greatest difficulty in the way of constructing a complete theory of the evolution of species. This difficulty was felt by Darwin, and has, if anything, become more striking with the development of modern genetical theory. One solution, has, it is true, been developed. It now seems clear that, in certain cases, allopolyploidy may be effective in producing a new fertile form that is cross-sterile with its parents. This solution is, however, certainly a special one, applicable only to hermaphroditic organisms. There is no recognized general scheme, applicable to separate-sexed forms, that can be pictured in detail. It is the purpose of the present note to suggest such a general scheme.

It was pointed out by Fisher (1930) that, if a species is supposed to be broken up into two more or less isolated groups, and the hybrids between these two groups are relatively infertile, then any genes tending to decrease the cross-mating between the two groups will be selected. This is because cross-mating, since it leads to the production of less fertile offspring, will lead to a decrease in the potential ultimate reproductive value of the cross-mated individuals, as compared to those that mate within their own group. In other words, intra-group matings will, if we consider generations later than the first one, produce more descendants than will inter-group matings—and selection will therefore decrease the proportion of the latter.

On this basis, sterility of the F_1 comes first, and leads to an accumulation of genes that prevent cross-mating. The alternative assumption, more often made, is that cross-mating is prevented in some manner, and sterility of the F_1 follows as a more or less incidental consequence. This latter view has always remained rather vague, and presents serious logical difficulties when one attempts to make it more precise. There seems to be no good reason why the hybrid should become sterile, yet sterility of the hybrid is one of the most wide-spread characteristics of distinct species.

There is also a logical difficulty about the view that sterility of the F_1 is primary. For, in this case, there must have been a fertile F_1 present in the beginning. If A and B give a sterile hybrid, how can B be derived from A, unless self-fertilization be assumed? There are two ways of escaping this difficulty: One may suppose the hybrid not to be completely sterile (in which case, as the sterility is decreased, it becomes easier for the condition to become established, but its effectiveness in leading to failure of cross-mating decreases), or one may assume that the sterility arose by two steps. In the latter case, one may assume two pairs of complementary genes, Ss and Tt, such that Ss Tt is sterile, but SS tt, Ss tt, ss TT, ss Tt, and ss tt are

fertile. The derivation of the two cross-sterile homozygous types SS tt and ss TT then involves the production, at some stage, of a type (ss tt) that is fertile with both (see elaboration by Dobzhansky, 1937).

Fisher's analysis is based on the assumption that the two diverging groups have become adapted, through selection, to different conditions (usually geographical), and that the hybrids between them are less well adapted to either set of conditions.

There are, however, certain other systems which will lead to the same type of situation, without any difference in the selective agencies acting on the two populations—without any initial differences between them in gene-frequencies. Perhaps the simplest example is that of reciprocal translocations. Here the heterozygote has a reduced fertility, owing to the more or less frequent occurrence of "irregular" segregation. The smaller the pieces of chromosome exchanged, the greater is the frequency of irregular segregation—but also the greater is the chance that some of the products of such irregular segregation may have some degree of viability and fertility (since they will carry duplications or deficiencies for fewer genes). It is presumably for this reason that small, rather than large, translocations appear to have become established in the phylogeny of *Drosophila* species (Sturtevant and Tan, 1937). This system suffers from the disadvantage referred to above—the greater the sterility of the F_1, the greater the difficulty of establishing the translocation in the first instance. It has been shown by Dobzhansky and Tan (1936) that several small translocations have in fact occurred in the course of the differentiation between *Drosophila pseudoobscura* and *D. miranda;* accordingly this method must be considered as a possible one, though the inversion system now to be discussed seems more likely to be of general occurrence.

Another type of chromosome aberration that should have similar effects is furnished by inversions. If two chromosomes differ by a single inversion within an arm, there exists a mechanism such that there is only a negligible effect of crossing over on the fertility of the heterozygote (Sturtevant and Beadle, 1936). If, however, an inversion is followed by a second one not greatly different from it, the situation is otherwise. For example: suppose a chromosome ABCDEFGH is inverted to form AGFEDCBH, and the latter in turn gives rise to AGCDEFBH—it being assumed that the spindle-attachment is at A or H. A heterozygote carrying the first and the third of the above chromosomes may give crossing over in the CDEF region, and the chromatids resulting from such crossing over (one carrying two B's, no G, the other carrying two G's, no B) will pass to the functional egg cells at random (Gershenson, 1932; Sturtevant and Beadle, 1936). The result is that such a heterozygote has a definitely decreased fertility. In the limiting case the decrease will amount to 50 per cent.

Such a system is not imaginary. The properties outlined have been experimentally determined; and the existence of inversions in wild populations is now known to be frequent and widespread. It may be concluded that they have in fact often led to the production of relatively infertile combinations. Given these facts, there is present all that is needed for the initiation of the selective process outlined by Fisher. It is possible to postulate such an origin for the two races (A and B) of *Drosophila pseudoobscura* that give sterile F_1 hybrids. These two races do in fact show some degree of sexual isolation; and the most frequent sequence in the third chromosomes of race A

(Arrowhead) is so related to the most frequent one of race B (Klamath) that crossovers of the type described should be relatively frequent (Dobzhansky and Sturtevant, 1938).

This case illustrates a difficulty encountered in such an analysis as is here attempted. The male hybrids between the two races are completely sterile, yet the postulated mechanism does not permit complete sterility at the initial stage. In fact, it is clear that in organisms that cannot self-fertilize, this must usually be so, as pointed out above. There thus arises a difficulty which was recognized by Darwin: the most that can be supposed in the early stages of differentiation is that the hybrid is relatively infertile. This infertility must be supposed to increase up to complete sterility—yet at first sight it seems that a decrease in fertility is precisely what selection cannot accomplish.

I have recently discussed (*Essay II* of this series) a case in which sterile individuals must be supposed to have increased in frequency because of selection. The general method invoked in that case may be so modified as to apply here also—namely, we may suppose that the area between the two incipient forms is occupied by a series of semi-independent small sub-populations, only occasionally interbreeding and ultimately competing with each other.

Within a mixed population, the hybrids themselves, when they do breed, will produce up to half their offspring like themselves. Therefore such fertility as they possess will be a source of new semi-sterile individuals. Selection will operate to increase the fertility of this class; but within a small sub-population, the chance establishment of any genes that decrease still further the fertility of the hybrids will increase the efficiency of the sub-population as a whole, and will give it an advantage over the competing sub-populations. Under these conditions, then, it may be supposed that the fertility of the hybrid may be decreased, while the frequency of crossing of the two races is still decreasing.

It is thus possible to formulate a scheme for the origin of interspecific sterility, starting with Fisher's demonstration that races giving a relatively infertile hybrid will tend to accumulate genes that tend to prevent crossing. Appropriate use of Wright's (1932) idea of competing sub-populations makes it possible to see how the infertility can be increased by selection. Inversions and small translocations furnish ever-present materials for initiating or for increasing such infertility of the hybrids—though it is clear that gene mutations must also be concerned, since not all interspecific sterility can be attributed to chromosome rearrangements (see Dobzhansky, 1937).

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that The Quarterly Review of Biology *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* The Quarterly Review of Biology, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

OF HUMAN FOLLY

Being a discussion of *The Symbols of Government* (1935. Pp. vii + 278), to be referred to hereinafter as SG, and of *The Folklore of Capitalism* (1937. Pp. vii + 400) to be referred to as FC; both these books having been written by *Thurman W. Arnold* and published by the *Yale University Press, New Haven, Connecticut.*

By Raymond Pearl, Department of Biology, School of Hygiene, Johns Hopkins University.

I

A primary objective of human biology as a scientific discipline is to find out as much as possible about how human beings behave and why they act as they do. Apparently a considerable number of persons are of the opinion that Mr. Thurman W. Arnold, who is a professor of law in Yale University and at the moment also a higher official of the federal Department of Justice, has made an important contribution towards an understanding of human behavior in these two books. So widely has this view prevailed that pressure has been exerted by the readers of the Quarterly Review of Biology to have the case examined critically and reported upon in the pages of this family magazine. This we gladly do in the spirit of helpfulness that has ever been our editorial purpose and guide. But beyond this altruistic and humanitarian motivation there is a further, and in our view more important, consideration. It is that in the future it will come to be more clearly recognized than is now the case that in the most complex patterns of human behavior—social, economic, legal, and governmental—underlying biological factors and forces, consequent upon the fact that man is first of all an animal, play a rôle of the very foremost rank of importance. The influence of these purely biological factors and forces will have to be justly appraised before any really penetrating understanding and interpretation of human actions can be reached. The biologist will in time come to take his part and place of equal right in the discussion of matters that have hitherto been regarded as the exclusive territory of sociologists, economists, and lawyers. This idea is in no sense novel—Aristotle was well aware of it, and Thomas Henry Huxley and Francis Galton made important contributions in conformity with it. But now it is rapidly gaining added momentum. This is not solely because it has come to be recognized as a fundamental element in the preaching and practice of modern psychiatrists, and notably Freud. Other forces are at work. During the last fifty years, and particularly in the last twenty-five, the mass alterations of human behavior and attitudes have made it transparently clear that the older modes of thought in sociology, economics, and law are not as helpful as could be wished. These disciplines tend more and more every day

to turn to the biologists for aid in their troubles. So it seems probable that few people will be shocked and most people will think it entirely natural and proper that books dealing in the concrete sphere with matters of law, politics, and government should be discussed in a biological journal.

II

The discussion of these two books before us may well begin with a consideraton of their more general characteristics, after which we can get on to particulars. And let the keynote of the composition be C-majorish—general approbation and appreciation of the cleverness, wit, and humor of the writing. Arnold says (SG, 257): "The keenest objective observations about government in the past has [*sic*] been spoken by satirists and humorists and instantly recognized by the mass of people who laughed with them." In these volumes he has used this technique of exegesis with notable skill, and almost uniformly throughout. Many a chuckle and some belly-laughs are neatly stowed away in the cargo of these books. Only fairly infrequently is the error of laboring the wit to force the chuckle conspicuous.

The theme of the books is the folly and stupidity of mankind, in respect of its behavior about matters in the realms of sociology, economics, law, and government. Making fun of man's foolishness is, of course, a very old story. Of all literary formulas it is probably, on the whole, the surest-fire laugh-getter. Furthermore its power to make for the practitioner a reputation of realizable lifetime asset value at its lower levels of skill, and in perpetuity at the higher levels, is not to be under-estimated. Lucian, Rabelais, and Voltaire, to name but a few out of a longish list, did well for themselves by poking fun at the stupidity and folly of their fellow-men. The reason for the unfailing success of the formula is well understood. It embodies the universal and all-embracing sop to human vanity. Each and every reader of writings embodying the formula identifies himself with the author, and says, in effect: "Isn't it good to be different from these silly, comic jackasses and boobs so justly depicted?" Everyman tells all his friends at the club that the author is a devilish clever chap and that they must get the book and read it at once. His sound confidence that every one of them will react to the formula as he does demonstrates at one and the same time both the truly protoplasmic profoundness of his understanding of human nature in this respect, and the dependably widespread failure of human beings to think of the statistical implications of simple ideas.

It seems certain that many persons think that what these two books say about government and cognate matters is new and important. At least the jacket blurbs on both are calculated to convey this impression. But actually, as Prof. Arnold himself says (SG, 257): "Certainly there are no new ideas about government in this book which were not familiar to Rabelais, or to Swift, or to . . . countless others." If the author is clear on this point his readers should also be, and recognize that the significance of these books, insofar as they are important at all, lies in the fact that they give a fresh description, analysis, and logical demonstration of the stupidity and folly of mankind with respect to another specified frame of reference or universe of discourse.

The particular kind of folly that is abhorred, ridiculed, and objurgated is again an old, well-known, and nearly universal one—the inadvertent confusion or deliberate substitution of abstractions for concrete realities in the conduct of life and affairs. The abstractions are held to take on the qualities of symbols and to lead to elaborate mythologies and folklores that guide and determine attitudes and actions, while phenomena very different from what is envisaged in the mythology and folklore are actually occurring. "When men are confronted with a contradiction between their myths and reality, they have only two recourses. The first is ceremony, drums, and oratory. The second is reason and dialectic." (FC, 115). So SG and FC are devoted to showing in rather pedantic, repetitive detail how idiotically comic and at the same time tragically inhumane and impractical in their results and effects are ceremony, drums, oratory, and reason.

The two books are substantially identical in content. Either one contains no ideas not in the other, though the examples used to illustrate poor humanity's follies are to a considerable extent different in the two books. The second (FC) is constructed after a more popular pattern, chiefly by omitting discussions of more highly technical legal situations that were very interestingly employed as illustrations in the first (SG). The effectiveness of this slight alteration of technique has been reflected in the comparative sales of the two volumes.

One other general characteristic must be mentioned. Any shrewd person who would exploit to its fullest the formula that the follies and stupidities of everybody but himself and his particular friends are funny, realizes that he must be careful to maintain at all points his detached urbanity and amused indifference. Especially the more notable practitioners of this art in the past have been extremely wary about any overt expression of an interest in the improvement of this imperfect world. The earnest uplifter and the brilliant satirist have rarely been amalgamated in one and the same identical corporeal frame. On this point the present treatises fall the merest shade of a thought of a hair's breadth short of the perfection achieved in *Gargantua* and *Pantagruel*, let us say. SG and FC are cagey books, to be sure—a chapter subhead (of Chap. XIII of FC) reads "In which the author plays safe and refuses to be specific"—but nonetheless there indubitably emerges, as the reader goes on, clear evidence of *le pathétique désir d'un monde meilleur*. While there is never anything so crass as the passionate for-God's-sake-let's-do-something attitude, it is made apparent in various subtle ways that if sufficiently urged the author might be willing to sacrifice his preference for lofty critical detachment, and show us how to fix it so that the government, the courts, business, and things in general will not be so funny or so stupid as they have been. In fact his present activities in Washington may not unreasonably be taken as evidence that he has already been seduced by such a siren voice. As Lecky pointed out long ago: "Impelled by a species of moral gravitation, the inquirer will glide insensibly to the system which is congruous to his disposition."

III

Careful reading of the two books indicates clearly enough the broad outlines of the technique by which Prof. Arnold would improve this poor idiotic country of ours. We are started from the position that government as it now is, and particularly as it was before 1933, is bad and wrong because we mistake symbols for reality; are guided by myths; and try to make governmental actions conform to principles instead of to political practicality. Then certain broad ideals for a more perfect kind of government are advanced (and re-advanced), explained (and re-explained), and advocated (and re-advocated). These ideals are fairly embodied in the following statement: Let government be frankly and overtly by *men* with skill for political *organization* rather than by principles and reason, and let it be freed from restrictive legal, social or economic symbols and myths such as are embodied in courts for example, and it will almost if not quite certainly be automatically both *practical* and *humanitarian*. This statement of the essential constructive message of the two books is more bald, specific, and concise than their own more tediously verbose amplification of the matter. But it seems a just summary of the position. The essentials in it are the four words italicized: *men*, *organization*, *practical*, and *humanitarian*. Prof. Arnold apparently sets great store by these words and the ideas they embody. And like most writers of his ilk nowadays he is much interested in semantics generally, especially semantics for other people. Indeed there is a whole chapter (VII of FC) devoted to *The Traps Which Lie in Definitions and Polar Words*. "Justice" and "injustice" are typical polar words.

> The battle between justice and injustice is a . . . struggle. It creates activity. It leads to change. It also leads to civil wars. What we call 'progress' is a consequence of this activity, as well as what we call 'reaction.' Our enthusiasms are aroused by these words and therefore they are excellent tools with which to push people around. Both the Rebels and the Loyalists in Spain are fighting for justice.

That is what enables them to kill so many people in such a consecrated way.

Since justice is a nice word, we refuse to apply it to people who are struggling for things we do not like. The pacifist will refuse to admit that any war can be a war for justice. The born fighter will say that men who refuse to fight for justice do not really care for justice at all. Each side gets morale from the use of such terms and obtains the confidence necessary to make faces at the other side, knowing that God is with him. However, these polar terms are purely inspirational. They are not guides. Each side always claims to have 'justice' on its side. Even organized criminals fight each other in the interest of justice.

All this is very fine and smart, of course. But are *organization*, *practical*, or *humanitarian* any less "polar" or inspirational words than *justice*? One fears not.

The general idea that practical politicians will operate government sensibly and effectively if not too much hampered by legalistic mythology is not precisely a new one. Indeed it has been tried at various times and places. And it has in fact worked moderately well over fair periods of time and again in various places. But, of course, what has hitherto always eventually disturbed the even and placid tenor of its way has been the development of one or another of these unfortunate myths and symbols by the foolish people—myths about the desirability of freedom and liberty; erroneous symbolic notions that practical politicians were not benevolent even though they liked to be called Great Humanitarians, but rather were damned selfish rascals bent only on keeping the power they had; and other similar delusions to which common folk are addicted.

But in an ideal point of view this difficulty could easily be got around, as is shown in SG, 232–236 *passim*.

From a humanitarian point of view the best government is that which we find in an insane asylum. In such a government the physicians in charge do not separate the ideas of the insane into any separate sciences such as law, economics, and sociology; nor then instruct the insane in the intricacies of these three sciences. Nor do they argue with the insane as to the soundness or unsoundness of their ideas. Their aim is to make the inmates of the asylum as comfortable as possible, regardless of their respective moral deserts. In this they are limited only by the facilities of the institution. It is, of course, theoretically possible to treat the various ideas and taboos which affect modern society, just as the alienist treats the delusions of his patients as factors which condition their behavior. This precludes any classification into sound or unsound theories.

The advantages of such a theory for purposes of thinking about government are that we escape the troublesome assumption that the human race is rational. We need not condemn policies which contradict each other solely on the ground that the action of government must be logically consistent. We need not constantly worry about permanent cures, and discard day-to-day policies because of their effects on the morale of the irrational people we are governing in the future. We need not delay such necessary undertakings as public relief because we are worried about their effect on the character of the recipients. We need not compel persons on relief to pauperize themselves and surrender the insurance policies which may afford future relief to their children because of a moral notion that no one is entitled to relief who is not a pauper. The theory eliminates from our thinking the moral ideals which hamper us wherever a governmental institution takes practical action—ideals which create the necessity of a *sub rosa* political machine.

Such a theory allows us to realize that cultures do not change because of the imposition of forms of government, and that the real contribution of new theories can only be to provide a faith which permits men to do practical and humanitarian things in government with confidence that they are not leading to some undefined danger.

The concept of government as an insane asylum liberates us from the notion that wise men think up principles and schemes of government for their duller fellows to learn and follow, and that thus social change is accomplished. It frees us from the notion that "thinking men" decide between the relative merits of communism and capitalism, and choose the better form. Finally, the theory is based on a humanitarian ideal which seems to be indestructible in the march of society—the ideal that it is a good thing to make people comfortable if the means exist by which it can be done.

Beautiful as this scheme is shown to be, it is reluctantly admitted (SG, 236) that "it will never work as a general political theory. Its realism is too apparent, as also is its implied scorn of the human race. The reasons for its failure are the reasons for the failure of all commonsense theories of government in a rational world." After this plaintive, wistful, and pathetic sigh for the *monde meilleur* the author goes on to list and discuss these reasons, but oddly does not include among them that simple one which might naively have been thought importantly obvious, namely that we are not all crazy yet.

IV

Even though the insane asylum idea probably will fail us practically as Prof.

Arnold suggests, all is not lost. A socially constructive thought is offered that may turn the trick (SG, 263, 264).

> The fundamental social axiom of the past was that man, by working only for his personal profit, in the long run produced the most ideal social results. Of course this profit motive had to be checked and balanced against its own excesses by law, by ethics, and by religion. But thus curbed it was part of nature's great plan. Attempts to interfere with it led to disaster. Great institutions like the law and the church, representing contradictory ideals, had to be carefully insulated from control of practical affairs by devices like trial by combat. And thus arose a spiritual government scattered between Washington and the various state capitols, and a temporal government scattered between New York and the various state financial centers, the one representing the great ideals and the other the fundamental axioms of social control.
>
> We suggest that the formula of the new social philosophy which is appearing may be the fundamental axiom that man works only for his fellow man; that it is *this* tendency which must be curbed by law, ethics, and common sense, so that there may be incidental room in the system for the man who works only for personal gain, just as there was incidental room in the old economic creed for the humanitarian. Under the profit creed the chief danger was from well-meaning but impractical humanitarians. Under the new creed the chief danger will be from well-meaning but impractical profit takers.
>
> Sometimes in clear outline, sometimes in strange and distorted and destructive forms, the new conception of man as a creature who does not work for himself is appearing all over the world. In Germany and in Italy the normal man is supposed only to work for his fellow countrymen, and the difference between social organization in peace and war has faded. In Russia the normal man is thought of as one who works for the toiling masses of the world. In all of these countries the axiom that the normal man in the long run works only for his own profit is put down as dangerous radicalism. Fanatical devotion to this single ideal is such that it makes human liberty an unimportant value, and even kindness is stifled for purely humanitarian motives.

This idea of a society in which men work only for their fellow men is extraordinarily interesting to the biologist. For elaborate social organizations of precisely this sort exist here and now in great abundance and variety. Furthermore they have been thoroughly studied by competent biologists. They are found in highest and purest development among the termites, with illuminating variants among the ants and other social insects. It seems odd that Prof. Arnold missed or neglected the opportunity to mention this, and to develop more richly the diverse aspects, implications, and consequences of this social utopia from the experience of the termites. Their social organization has really solved so many of the problems that vex us, like agriculture, unemployment, housing, the Supreme Court, and so on, that it should be helpful to any great humanitarian to study the literature of the subject. Furthermore there is little doubt in the minds of many thoughtful human biologists that the present evolutionary trend of human society is definitely towards the termite plan, or that the rate of progression towards this stable goal is steadily accelerating.

V

The primary purpose of these two books under review is not, however, to advance a theory of government, to plan utopias, or to be constructive generally. It is rather to demonstrate by argument and example that the overwhelmingly important, if not indeed the sole, causes of the present ills of the world are the folly and stupidity about the myths, symbols, and folklore that have determined social, economic, and governmental action. These ills include social unrest, economic depressions, unemployment, war, and various other mass discomforts and disabilities. Prof. Arnold unquestionably makes a strong and convincing case that the myths and symbols are in some part causal elements in the picture. The case is not, however, convincing that they are the sole causes. Nor is it entirely convincing that they merit the first rank of importance in the premises. There are two considerations that need brief discussion here. The first is the general one that while, on the one hand, the present exacerbation of the world's ills is *of the present time*—say of the first third of the twentieth century—and is of a severity and widespread distribution over the face of the earth unparalleled in recorded history; it is equally true that, on the other hand, the folly of mankind in respect of myths and symbols *is not peculiar to the present time*, but on the contrary is not only as old as man himself but has been in continuous and universal operation as long as he has existed. Yet

as a matter of recorded history there have been quite long periods and various places where these myths and symbols did not produce the sort of ills from which we are now suffering. Instead things went along pretty quietly and peacefully, people were comfortable and happy, and the arts and sciences progressed. Such facts cannot be neglected in considering the myth-symbol idea as primary cause of the present world situation.

The second, and more special, consideration is that two factors in the case that seem to the human biologist of the first rank of importance as contributory causes along with myths and symbols, are completely neglected in these books, even to the point of not being mentioned at all. The first of these is the extraordinary growth of world population since roughly the middle of the seventeenth century. In the short period of 300 years or a little less the numbers of human beings living on the face of the earth have multiplied nearly *five-fold*. This has led to an average population density over the whole earth at the present time of approximately 41 persons per square mile of land area. And this means literally *all* the land area—good land, completely worthless land impossible for human habitation and sustenance, and mediocre land. There are solid grounds for the view that this world population density is in itself, directly and indirectly, a potent causal factor behind the world's present unrest and ills. The evidence for this view has been discussed by the writer elsewhere (*The Natural History of Population*, Oxford Univ. Press, 1938) and cannot be further elaborated here. The only point here is that it cannot wisely be left out of the picture Prof. Arnold essays to paint.

The other factor that is neglected is basically a political one. It is that generally, and with some admitted but relatively unimportant local exceptions, the trend of social, political, and economic evolution since the early part of the nineteenth century has been towards ever greater and greater *concentration of power* in the hands of very small groups relative to the total number of human beings struggling to survive on the earth. Of prime significance is the trend towards increased concentration of governmental power in central at the expense of local agencies. Economic power has similarly become more and more concentrated with the development of trusts, monopolies, great industrial organizations, and so on. All of this Prof. Arnold discusses. Some of it has his approval, especially central concentration of governmental power because there can thus be "practical common-sense" government. But the point that the human biologist would raise is that all and any concentration of power tends to cause human unrest and discomfort, because long and painful experience has demonstrated that no human being can be trusted with much power of any sort very long. If any axiom can be said to be demonstrated by experience it is that the more concentrated the power of a government is the more intolerable that government becomes as time passes. Every benevolent despot (and of course all despots are benevolent if you let them tell it) has shared Prof. Arnold's view that men are boobs and fools, only to have it revealed in the sad end that they can also be fighting fools.

VI

The evolution of law and government has been, in technical biological lingo, a continuing and progressive attempt at group adaptation to meet environmental difficulties consequent upon sociality. When numbers of men had to, or chose to, live together in a spatially limited territory, rules of conduct were devised and by common, if never quite universal, consent agreed to, in order to ensure that the group should be protected so far as possible from annoyance, abuse, and damage by individuals determined for whatever reason to be nuisances. This process has crystallized out laws and government, the latter being the mechanism to ensure the observance of the rules as effectively as might be. But sensible men have always been of the opinion that the least government was the best government. Now any historically informed person knows that this process, in common with all truly evolutionary processes of which we have adequate knowledge,

has been slow, inefficient, and characterized by an enormous amount of foolish and bungling trial and error. But it has steadily gone on, in fact. As the number of human beings living together increased, and the techniques of getting a living multiplied, the problems of rule-making and administration of the rules naturally become more and more complex and difficult to solve, and the imperfections of the attempts more apparent and troublesome. It is not difficult to pick out and emphasize, as Prof. Arnold has done, the conspicuously bad and foolish elements that have accrued as this long and muddled process has gone on. But they do not quite make the complete picture. Men are foolish fellows, of course, and have made a lot of mistakes. But they have corrected a lot of mistakes too. And if man can manage to continue to survive as a species, about which there is always some doubt, a lot more mistakes will very slowly but surely be corrected. For such is the normal way of evolution.

What is often forgotten or overlooked is the terrible slowness of organic evolution. Man's most recent acquisition evolutionally is that part of his brain that is associated with conscious thought and the capacity for reasoning. In the long eons of time that have been required in the evolution of living things from the simplest protists to man it was just a little while ago that forebrains began to bulge out to make highbrows. Is it any wonder that men are still foolish in the head? But let it not be forgotten that in the enormously longer time that it had to do it in man's body has achieved a wisdom that is superb in its precision, accuracy, and efficiency. It is reasonably to be expected on the basis of past experience, again with the proviso that he manages to survive as a species, that in the passage of many, many thousands of years the wisdom of man's mind may become more nearly equivalent to the wisdom of his body than it now is. The patient, the meek, and the humble—the "foolish," in short—will probably manage somehow to struggle along and to view this prospect with some degree of equanimity because they will sense that there is nothing very immediate or startling that they can do about it. Bright and witty messiahs, whom we shall long have with us, will continue to be irked because the process is so slow, so inefficient, and neither "practical," "political" nor "humanitarian."

BRIEF NOTICES

EVOLUTION

A History of Land Mammals of the Western Hemisphere. *Revised Edition Rewritten Throughout.*

By William B. Scott. Illustrated by R. Bruce Horsfall and Charles R. Knight. The Macmillan Co., New York. $7.50. $8\frac{1}{2} \times 5\frac{1}{2}$; xiv + 768; 1937.

It was just a little more than 60 years ago that the author of this volume together with the late Henry F. Osborn and Francis Speir, Jr. planned and carried out their first palaeontological expedition into the Bad Lands of southwestern Wyoming. They entered an almost virgin field but one abundantly rich in material. In both the western part of North America and in southernmost South America there had been preserved marvelous series of records of the successive assemblages of animals that once dwelt in these regions. The present book (first appearing in 1913 but now entirely rewritten, except for the chapter on the skeleton and teeth) brings together all of the important links in the ancient history of mammals as told by fossilized forms and so far deciphered by the painstaking efforts of various investigators, of whom Professor Scott is one of the foremost leaders.

The book, which will long be a classic, should be in all biological libraries for it will be invaluable to teachers as well as students in this field. It will also be of great interest to the general reader. Fossilized animals were never given common names, therefore Professor Scott has used throughout the technical nomenclature, but this can hardly be considered a hindrance to the enjoyment of the book since the numerous remarkably fine illustrations (420 line drawings and photographic plates—those that are new being the con-

tributions of R. Bruce Horsfall) aid materially in comprehending the text. A detailed index is included.

THE NATURE AND MEANING OF EVIL AND SUFFERING AS SEEN FROM THE EVOLUTIONARY STANDPOINT.

By C. J. Bond. H. K. Lewis and Co., London. 1s. $8\frac{5}{8} \times 5\frac{1}{2}$; 29; 1937 (paper).

An interesting discussion of two theses: (1) that evil is a relative term, and is caused by strong individualism, and (2) that suffering is a result of our ethical evolution not being able to keep pace with cosmic evolution. The conclusion is that evil and suffering in the world will become extinct only when we have a common universal race, a common religion, and a common system of education. The text is developed in a logical manner, and presents much evidence in support of the basic ideas. However, there have been very few books written which can boast, as this one can, that more than 75 per cent of its paragraphs contain only one sentence.

ENTSTEHUNG DER MENSCHENRASSEN.

By Hans Weinert. Ferdinand Enke Verlag, Stuttgart. RM. 17. (paper); RM. 18.80 (cloth). (Outside of Germany, 25 per cent less). $10 \times 6\frac{1}{2}$; viii + 313; 1938.

This little book concerning the origin of human races contains little that is actually new on the subject, but it is an excellent summary of all that is known about prehistoric man from the standpoint of physical anthropology. Five chapters are devoted to Neanderthal and pre-Neanderthal races, one chapter each to Diluvian man, Mesolithic man, Neolithic man, and present day races, with special emphasis on present day European white races. There is a bibliography and index.

GENETICS

HEREDITY AND POLITICS.

By J. B. S. Haldane. W. W. Norton and Co., New York. \$2.50. $8 \times 5\frac{1}{2}$; 202; 1938.

In this book, based on a series of Muirhead Lectures at the University of Birmingham, Professor Haldane gives a popular exposition of our present knowledge of human genetics and of the bearing on such measures as compulsory sterilization and anti-Semitic legislation. When an abnormal condition is due to a dominant gene that manifests itself in 100 per cent of cases and early in life, sterilization would cut off all hereditary cases of the defect, leaving only those cases due to mutation. Where the condition is recessive sterilization would not produce a noticeable effect in less than thirty or forty generations, whereas the prohibition of cousin marriages would reduce the incidence of the abnormality appreciably.

Although it is often stated that the children of two mental defectives are all defective, it was found that in Birmingham of thirteen children both of whose parents were defective "only one was pronounced defective in 1933 and another one may possibly be registered by now." Of 345 children at least one of whose parents was defective only 7.5 per cent were defective, 18.5 per cent were backward, 71 per cent were normal and 3 per cent were above the average. Professor Haldane concluded "that the sterilization of all mental defectives would probably cut down the supply of mental defectives in the next generation by something of the order of 10 per cent." If such a reduction is thought sufficient to warrant measures to prevent mental defectives from having children, he favors segregation in institutions as more humane than sterilization.

PHYSIOLOGICAL GENETICS.

By Richard Goldschmidt. McGraw-Hill Book Co., New York. \$4.00. $9 \times 6\frac{1}{8}$; ix + 375; 1938.

With the ever growing interest in the field of genetics, and with the increasing amount of experimental work and the development of new methods in the study of heredity has come the need for someone to synthesize the fundamental findings of the different workers into a comprehensive volume. Just such a synthesis Goldschmidt has attempted in this volume. He not only has had long experience in experimental genetics, but he has the rare

ability to combine his own work with the work of others into a very interesting and readable book. The author is entirely justified in stating in his preface that "Not only has this material been assembled and reviewed, but an attempt has been made to organize it into the skeleton of a future science of physiological genetics." The text is quite technical throughout, but it is assumed that the reader has enough background in genetics and embryology to understand the fundamental principles concerned even if he does not understand all the technicalities. The 44 page bibliography, the author index, and the subject index add to the usefulness of the volume. All geneticists will not agree with some of the theoretical positions taken.

Faktorenkoppelung und Faktorenaustausch bei Normalem und Aberrantem Chromosomenbestand. *Probleme der theoretischen und angewandten Genetik und deren Grenzgebiete.*

By Wilhelm Ludwig. Georg Thieme Verlag, Leipzig. RM. 11. $8\frac{1}{4} \times 5\frac{3}{4}$; 245; 1938 (paper).

The problem of crossing-over and exchange of parts between two or more chromosomes is one of the most important and complicated chapters in modern genetics. The author gives a very concise insight into these problems—the genetic theory as well as the cytological fundamentals. The presentation is hardly suitable for beginners in biology and genetics, but the well-grounded student will profit from this special review which covers almost all of the newer investigations. As mathematical symbols play an important rôle in genetic thinking these can not necessarily be avoided in some of the discussions. There is an exhaustive list of the literature up to 1937, and indices of authors and subjects.

Spontane und Strahleninduzierte Mutabilität.

By H. Stubbe. Georg Thieme Verlag, Leipzig. RM. 6.80. (Outside of Germany, except Palestine, 25 per cent less). $8\frac{1}{8} \times 5\frac{3}{4}$; 190; 1937 (paper).

This little book is intended as a review and summary of the most recent work done in the field of mutations. After an introduction to the mutation theory, the author discusses at some length different types of spontaneous mutations, their effects, their manner of origin, dependence on temperature, etc. By far the greater portion of the book however is devoted to X-ray mutations. There is a lengthy discussion of H. J. Muller's contributions on the methods, results, and generalizations that can be drawn from X-ray genetics, the production of chromosome and gene mutations from short wave rays in animals and plants, and practical results particularly for the field of botany. There is a 20 page bibliography and an index.

Fortschritte der Erbpathologie, Rassenhygiene und Ihrer Grenzgebiete. *I. Jahrg., Heft 1 and 2.*

Edited by Johannes Schottky and Frhr. von Verschuer. Georg Thieme Verlag, Leipzig. Subscription price RM. 16. plus postage. $10 \times 6\frac{3}{4}$; 134; 1937 (paper).

This new journal is concerned mainly with pathological genetics connected with race hygiene. A few of the articles in the first two numbers of Volume I have the following titles: Inheritance of intelligence and character, by K. Gottschaldt; Selection, by H. Wülker; Inventory of inheritance, by H. Schade; Discoveries in the inheritance of schizophrenia and cyclothymia, by H. Luxenburger; Epilepsy, by H. Geyer; and Inheritance of dental malformations, by F. Proell.

The journal has so far managed to accept articles of real scientific value and free from any political propaganda. This is unusual for a publication from Germany dealing with race hygiene and it is to be hoped the editors will continue to select articles of such calibre.

GENERAL BIOLOGY

The Axiomatic Method in Biology.

By J. H. Woodger. With Appendices by Alfred Tarski and W. F. Floyd. University Press, Cambridge; The Macmillan

Co., New York. $3.75. $8\frac{1}{2} \times 5\frac{1}{2}$; x + 174; 1937.

In 1930 and 1931, J. H. Woodger published in Q. R. B. (Vol. 5 and 6) a noteworthy essay in three parts entitled *The "Concept of Organism" and the Relation between Embryology and Genetics* in which modern symbolic logic was applied to biology. This excellent paper has now been followed by a book in which the first steps towards treating genetics, embryology, and taxonomy from an axiomatic point of view have been taken. The result is a work of signal importance in so far as it not only inaugurates a new subdivision of biology but also applies to biology a philosophical instrument which raises the philosophy of biology, and indirectly the philosophy of science, to a new position of eminence. Woodger has taken a long stride forward towards inculcating methods of exactness and rigor into biology, and representing the philosophy of science as the discipline of exact definitions, explicitly stated assumptions, and the rigorous deduction of logically related theorems. Thus as a biological work, *The Axiomatic Method in Biology* stands as the inceptor of a new biological science, while as a philosophical work it deserves a place of prominence as a guide and example to future endeavors in philosophical research among scientific topics.

The first two chapters contain a brief summary of those logical concepts and methods which are essential to an understanding of their subsequent application to biology. Though these are admirably presented, they are insufficient in themselves to prepare a reader, unfamiliar with symbolic logic, for the chief topic of this book. It should be recommended to such a reader that he first acquaint himself with Carnap's *Abriss der Logistik*, the influence of which is clearly displayed throughout Woodger's work.

THE HUMAN VALUE OF BIOLOGY.

By Johan Hjort. Harvard University Press, Cambridge. $2.50. $7\frac{1}{2} \times 5\frac{1}{8}$; xii + 241; 1938.

This absorbing little volume is a revision of a series of lectures given by the author before his students at the University of Oslo, and presents in a dramatic fashion a summary of his philosophy of life. Hjort's study of marine populations has led him to believe that much knowledge can be gained from a study of populations of lower forms which can, either directly or indirectly, be applied to human populations in our effort to bring about a better social order.

Historical evidence is presented to show that the different philosophies have contributed to the development of biology, but it makes the point that present day biology, to be of the highest human value, must be interpreted by biologists, and not by philosophers of pure reason, mathematics, or physics. Hjort summarizes his views in the idea that an ideal goal for any science, in fact, for any unit of society, is to aid in . . . "liberating those who suffer from want of intellectual activity in their work, the greatest of all social diseases." There is a bibliography.

LABORATORY MANUAL FOR GENERAL BIOLOGY.

By Leslie A. Kenoyer and Henry N. Goddard. Harper and Bros., New York and London. 75 cents. $10\frac{3}{4} \times 8\frac{1}{2}$; 73 + 27 plates; 1937 (paper).

LABORATORY DIRECTIONS IN COLLEGE ZOOLOGY. *Revised Edition.*

By Henry L. Bruner. The Macmillan Company, New York. $1.75. $8\frac{1}{2} \times 5\frac{3}{4}$; xiv + 163; 1938.

LABORATORY OUTLINES FOR ANIMAL BIOLOGY. *Revised Edition.*

By Michael F. Guyer and Halcyon W. Hallbaum. Harper and Brothers, New York. $1.75. $8\frac{1}{2} \times 7$; xvi + 273; 1937.

The first of these manuals (dealing with both plant and animal forms) is so arranged that by the removal of the cover it can be slipped into a loose-leaf notebook. A group of 27 plates showing line drawings, largely adapted from well-known texts, is provided for the student to label after his work with laboratory specimens is completed. The second manual emphasizes the morphology of animal forms. It fits in particularly well with Hegner's *College Zoology* (fourth edition), and offers adequate material for a full year's work. The last manual is more detailed than the other two. It is

intended to be used in conjunction with the senior author's textbook of *Animal Biology* and also provides a full year's work.

NEW INTRODUCTION TO BIOLOGY. (*Revised*).

By Alfred C. Kinsey. J. B. Lippincott Co., Chicago. $1.76. $7\frac{5}{8}$ x $5\frac{1}{2}$; xv + 845; 1938.

The present edition of this fine high school text follows very closely the content and organization of the earlier edition (Q. R. B., Vol. 9, No. 1). The body of excellent illustrative material, which has had minor omissions, alterations, and additions, is now in such form as to give the content of the text more unity and emphasis with regard to the relationship of structure and function, and to the relationship of organisms to their environment and to each other. The volume contains, as did the earlier edition, a list of select references, a fine glossary, and a complete index.

SOURCE BOOK OF BIOLOGICAL TERMS.

By Axel L. Melander. Department of Biology, The College of the City of New York, New York. $1.10. $8\frac{1}{2}$ x $5\frac{3}{8}$; vi + 157; 1937.

The layman will find this book informative, though esoteric; the classical scholar will find that brother scientist has steeped his nomenclature in mythological lore; the biologist will polish up his technical vocabulary along with his Greek and Latin; and the science student will be able to acquire standardized pronunciations and meanings with a technical vocabulary. Part I is a discourse on such problems as word sources, homologies, phylogeny, origins, emergence, derivations both mistaken and uncertain, and mispronunciations. Part II is an alphabetical list of the components of the biological vocabulary. A scholarly book.

BIOLOGY. *The Story of Living Things.*

By George W. Hunter, Herbert E. Walter and George W. Hunter, III. American Book Co., New York. $3.75. $8\frac{3}{4}$ x $5\frac{7}{8}$; x + 670; 1937.

The writers of this excellent elementary text have hit upon something new and appealing in their method of presentation. Every chapter has zest and vitality, and even the usually boring sections on classification have been handled with such precision and finesse that they captivate one's attention and consideration. The text is beautifully illustrated throughout, either diagrammatically or photographically, and each chapter is concluded with a brief bibliography on the subject presented.

LADY JULIA PERCY ISLAND, 1935 EXPEDITION. *Reports of the McCoy Society for Field Investigation and Research. No. 1.*

Proceedings of the Royal Society of Victoria, Vol. 49: 329–437, 1937.

Lady Julia Percy Island, lying off the south coast of Australia and appearing to represent a small and self-contained sample of the Australian environment, was selected as the site of the first season's investigations of the McCoy Society. The island proved to be unique, however, rather than typical when it was found to be purely volcanic instead of a portion of the old continental mass cut off from the mainland. This first report of the Society puts on record all findings made during the summer of 1935–1936 on the state of the balance of nature on this island, and includes lists of the plant and animal forms found.

INTERNATIONAL ASPECTS OF OCEANOGRAPHY. *Oceanographic Data and Provisions for Oceanographic Research.*

By Thomas W. Vaughan and Others. National Academy of Sciences, Washington. Free, (not available for general distribution). 12 x 9; xvii + 225 + 36 plates; 1937.

The purpose of this volume is to index the available literature on geophysical contributions to oceanography and to catalogue the institutions (and their resources) which are adding to the knowledge of the subject. Maps and tables

show the regions in which work has been done on serial sections of temperature and salinity in the different ocean basins, charting the bottom of the oceans, submarine earthquake epicenters, and results of maritime gravity research (1923–1932). The catalogue of institutions engaged in oceanographic work occupies about two-thirds of the book.

THE PENDULUM SWINGS BACK.

By Marvin M. Black. Cokesbury Press, Nashville, Tenn. $2.00. $7\frac{7}{8}$ x $5\frac{1}{2}$; 229; 1938.

The author of this verbose bilge is of the opinion that "science seems to have gone as far as it can with experimentation," and suggests that biology should now look for its further progress in such directions as telepathy and clairvoyance, the philosophical implications of Lamarckism, and the "soul in medicine."

HUMAN BIOLOGY

GEORGES DREYER. *A Memoir by His Wife. By Margrete Dreyer. With an Introduction by Sir Charles Sherrington. Basil Blackwell, Oxford.* 10s. 6d. net. $8\frac{1}{2}$ x $5\frac{1}{2}$; ix + 249 + 4 plates; 1937.

This book is a worthy memorial of a distinguished immunologist and physiologist. Georges Dreyer was a Dane by parentage and was educated at the University of Copenhagen, where he studied bacteriology and pathology under Salomonsen, and collaborated with Madsen in studies on diphtheria toxin and with Finsen on the biological effects of light. In 1908, at the age of thirty-four, he was elected to the Chair of Pathology at Oxford. During the war he developed an automatic apparatus for supplying the proper concentration of oxygen to flyers at high altitudes and supervised its manufacture for the British and American air forces. This work on the physiology of respiration led him to the study of "Vital Capacity"—the maximum amount of air one is able to expel from his lungs—and its relation to physical fitness.

Much of Dreyer's research was given to the development of more exact quantitative methods in biology and especially in immunology. Thus in his Copenhagen days he developed a standard technique of the Widal reaction with a sensitivity of 99.1 per cent and a specificity of 98.8 per cent. At Oxford he collaborated in research on a diagnostic test for syphilis that was one of the forerunners of the Kahn test. In his Harvey Lecture at Harvard he again championed the cause of biological standards.

> Now, the first question that comes to the mind is: What advantage do we derive from the use of definite standard units? and the answer is readily given: that science-workers of every country and every race in this way are enabled to understand each other for the simple reason that they speak the same language.
>
> This has already been achieved in the field of electricity by the universal acceptance of the well-known standard units, the ampère, the volt, the watt, the ohm, etc., while the civilized world is still divided on such important questions as the system of weight, measure and coinage.
>
> If the goal of a really close co-operation between medical research workers could be reached, the biological sciences would be put on an equal footing with those hitherto accepted as the "exact."
>
> The difficulty of fixing biological standards is only too often believed to be insurmountable because of the idea so widely taught and accepted that any great degree of accuracy in biological science is beyond our reach.
>
> That this difficulty, however, is more imaginary than real should not be doubted when one remembers that some of the fine tests in the "exact" science depend on biological functions, namely, those of sight and hearing. . . .
>
> Of course individuality and independence is admittedly the proper spirit in science and any measure that might tend to interfere with that freedom of thought and action is justly condemned. And as acceptance and use of biological standards often impose the adoption of a definite technique one easily understands the hesitation.
>
> Yet, the use of valid standard units can never really interfere with individual freedom. Hardly anyone, even the greatest of champions for individuality, could maintain that the universal acceptance of units like the ampère, the volt and the ohm has in any possible way interfered with the individuality and originality of modern investigators.
>
> On the contrary, research carried out without the use of these standard units would be lost to the scientific world at large because of the lack of information concerning the values involved. . . .

As will be seen, Dreyer accomplished important work in several fields of medical research. Yet the book deals not only with the scientist but with the man, his friendships, his zest for life. Here was a

man whom it was an inspiration and a delight to count as one's friend.

SEXUALITY IN THE SECOND DECADE. *Monographs of the Society for Research in Child Development, Volume II, Number 3. Serial No. 10.*

By Raymond R. Willoughby. Society for Research in Child Development, National Research Council, Washington. 75 cents. $9\frac{1}{8} \times 6\frac{1}{8}$; iv + 57; 1937 (paper).

SEXUAL MATURATION AND THE PHYSICAL GROWTH OF GIRLS AGE SIX TO NINETEEN. *Monographs of the Society for Research in Child Development, Volume II, Number 5. Serial No. 12.*

By Frank K. Shuttleworth. Society for Research in Child Development, National Research Council, Washington. $1.25. $9\frac{1}{8} \times 6\frac{1}{8}$; xx + 253; 1937 (paper).

DATA ON THE GROWTH OF PUBLIC SCHOOL CHILDREN (*From the Materials of the Harvard Growth Study*). *Monographs of the Society for Research in Child Development, Volume III, Number 1. Serial No. 14.*

By Walter F. Dearborn, John W. M. Rothney and Frank K. Shuttleworth. Society for Research in Child Development, National Research Council, Washington. $1.00. $9\frac{1}{8} \times 6\frac{1}{8}$; 136; 1938 (paper).

The first monograph is a review of those factual studies which, in the opinion of the author, "enable some generalization about adolescent sexuality." Under the headings of *Implicit Sexuality*, *Autosexuality*, *Para- and homosexuality*, and *Heterosexuality* the author presents data from numerous investigations regarding the onset of various types of sexual experiences, very seldom, however, giving any indication as to how the original studies were carried out and what reliability can be attached to the figures so copiously cited. There is a bibliography but no index.

The second monograph is concerned primarily with the statistical techniques applicable in an analysis of longitudinal data. Endocrine factors (time of first menstruation) and their relation to growth patterns were also studied. Altogether 3650 girls from Massachusetts schools were followed, with annual measurements as long as they remained in school. The measurements, in addition to information on the onset of first menstruation, included eleven physical dimensions, dental examinations, intelligence tests, school progress, etc. It was found that "interrelations of assumed endocrine stimulation, advent of menarche, and the timing of patterns of growth are very high." A large number of graphs and tables have been added to the text.

The third monograph presents in detail the repeated growth records acquired over a period of 12 years, on 1553 individuals, these cases being selected for completeness of data from the records of the Harvard Growth Study. The first third of the text is made up of explanatory notes on the tables that follow.

BIO-POLITICS. *An Essay in the Physiology, Pathology and Politics of the Social and Somatic Organism.*

By Morley Roberts. J. M. Dent and Sons, London. 15s. net. $9\frac{1}{4} \times 6\frac{1}{8}$; xv + 240; 1938.

This remarkable book deserves much more extensive and thorough discussion than limitations of space permit here. Under these circumstances we conceive it to be our first duty to recommend strongly and unreservedly to our readers that they get the book and both read and study it. It is a penetrating and realistic contribution of first rate importance to sociological theory.

Morley Roberts is known to most people only as a novelist. Actually he has been for the greater part of a long life a serious and profound student of biology, and an occasional writer in that field. In this book he develops to something like logical completeness the idea of human society as organism that Herbert Spencer dropped when he perceived that its implications were going to run painfully contrary to some of his most cherished prejudices. A strong case is made for the view that nations display the characteristics of loosely-knit low-grade organisms struggling to survive. It is further made clear that there is no evidence that intellect has played any significant part in the

growth, disorder, or decay of society—in social evolution, in short. Government is more soundly to be apprehended as a set of physiological responses to the internal and external environment in accordance with physical law than as an historically elaborated and intellectually sophisticated instrument.

The book is written with the clarity and charm expected from its author's high literary craftsmanship. It is adequately indexed and carries a soundly selected bibliography. Again we emphasize that it deserves the widest reading, and will repay the reader.

VÖLKER AM ABGRUND. *Zweite vermehrte Auflage.*

By Friedrich Burgdörfer. J. F. Lehmanns Verlag, Munich and Berlin. 3 marks; (outside of Germany) 2.25 marks. $9\frac{7}{8} \times 6\frac{5}{8}$; 76; 1937 (paper).

Burgdörfer adds another book to his already numerous publications on the dangers of the declining birth-rate for Western civilization. It is not overpopulation, he points out, but underpopulation that now menaces the highly civilized countries of Europe and European stock. The earth could feed 8–10 billions of human beings (instead of around 2 billions as the present population of the earth) and the United States is easily in a position to support 500 to 600 millions of men. But in almost all nations of Western civilization the birth-rate is rapidly decreasing. This is demonstrated by a series of tables and figures on population movement in recent years. The German government alone, since 1933, has checked this danger. After an almost uninterrupted decrease in births, the number of live births is increasing again, amounting in 1933 to 971,000, in 1934 to 1,198,000, in 1935 to 1,264,000 and in 1936 to 1,279,000. That means a birth-rate increasing from 14.7 per 1,000 in 1933 to 19.0 in 1936. However, that is no greater than the birth-rate was in 1928. At the turn of the century it was almost double in Germany, the absolute number being over 2 millions of live births a year. Burgdörfer explains the single methods of the government's economic and social policies in relief of marriages (*Ehestandsdarlehen*), but he furthermore wants to show that by far the more important was the will of reproduction among all groups of the population. So he demonstrates that not only the number of first-born (of the young marriages) have increased but also the second, third, and fourth-born (of the older couples). This publication, of course, has more the character of propaganda (edited as No. 1 in a series on political biology) than of scientific fundamentals. It is illustrated by tables and is indexed.

POPULATION PRESSURE AND ECONOMIC LIFE IN JAPAN.

By Ryoichi Ishii. University of Chicago Press, Chicago. $3.00. $8\frac{1}{2} \times 5\frac{5}{8}$; xix + 259; 1937.

Although this detailed study of the Japanese demographic and economic situation was completed before the outbreak of the present Sino-Japanese hostilities, it will probably be read in the light of that conflict. After remaining nearly stationary during the eighteenth and early nineteenth centuries the population of Japan has nearly doubled since 1872. As only about 15 per cent of the area of Japan is arable this rapid increase in population has resulted in a density of 27 persons per acre of tilled land as compared with 21 in the Netherlands.

Although the trend of the birth rate has been downward since about 1920 this is counterbalanced by a corresponding decline in the death rate. Although the former hostile attitude of the Japanese government towards birth control is changing, it is doubtful how soon this factor is likely to have an appreciable effect on population growth. According to various estimates the Japanese will have to feed from 15,000,000 to 20,000,000 additional mouths within the next generation. How can they do this? It is estimated that 5,000,000 acres can be added to the agricultural land of Japan. This, together with the proposed increase of acreage in the Japanese colonies and the probable increase in production per acre, will make Japan self-sufficient with

regard to her food supply. However, the cost of food production is relatively high. The best hope of taking care of the increase of Japanese population without reducing the standard of living seems to be a further industrialization of the nation. But an industrial nation poor in natural resources requires access to sources of raw materials as well as a market for its manufactures. The motives for the present conflict in China are therefore evident, but whether the Japanese adventure will solve her difficulties or add to them is not so plain.

AFRICA'S GOD. VII. EAST AFRICA. *Anthropological Series of the Boston College Graduate School, Volume II, Number 4.*

By Joseph J. Williams, S.J. Boston College Press, Chestnut Hill, Mass. $1.00. $9\frac{3}{4} \times 6\frac{1}{2}$; 54; 1937 (paper).

An interest in the Negro race, especially in the Jamaican Negroes and their beliefs, has led Dr. Williams to study the parent-stock in Africa. Since accurate observations of the African tribes by one individual is an impossible task, a questionnaire was broadcast "among those who were actually toiling along the required lines throughout the length and breadth of the Dark Continent." The answers, which are still being sent in by persons best fitted to understand each tribe (that is, by missionaries and government officials who have lived many years among the Negroes) are available at the Dinand Library, at Holy Cross College, Worcester, Mass.

The present monograph includes the answers of reliable workers concerning the tribes of Kenya and Tanganyika Territory in East Africa. The conclusions reached are that the tribes of the regions studied are monotheistic in their religious beliefs. The Wafipa of Tanganyika show positive signs of religious decadence, but, "vestiges of monotheism are found among the ancient beliefs of this tribe"; the monotheistic tendency, in general, "looks to a strong Hebraic infiltration that made itself felt all along the territory covered by the present paper."

There is also a brief historical discussion on the Hebrew exodus from Egypt and the Jewish influence on Africa thereafter, as a result of Jewish immigration and colonization.

Dr. Williams's paper is followed by an argument in favor of "Revelation and Primitive Sacrifices" by William J. McGarry, *S.J.*, President of Boston College.

CHINA AT WORK. *An Illustrated Record of the Primitive Industries of China's Masses, Whose Life is Toil, and Thus an Account of Chinese Civilization.*

By Rudolf P. Hommel. Published for the Bucks County Historical Society of Doylestown, Pa. by the John Day Co., New York. $5.00. $10\frac{3}{4} \times 8\frac{1}{8}$; x + 366; 1937.

This book is the result of an expedition to Central China financed and directed by Henry Chapman Mercer, that versatile historian and antiquarian of Bucks County, Pennsylvania. Its object was to investigate, photograph and describe the implements and methods of labor used in the rural districts, in order to throw further light upon the daily life, character and civilization of the Chinese people in the area studied. No generalizations as to tools or customs are possible in speaking of China.

The photographs and measurements of the implements, which fully illustrate the text, were difficult to take because of the prevalence of superstition and the extreme reluctance of the farmers to let their possessions be photographed. It is hard to realize that these heavy, crude, ancient looking tools are the present-day working equipment of millions of Chinese. There have been few changes in thousands of years. In every photograph and every detailed description of the laborious methods employed there is amazing evidence of the tremendous—almost superhuman—work involved, and the prodigal expenditure of this physical energy. There could be no more graphic way of depicting the traits of infinite patience and long-suffering of the Chinese people than in a study of this kind.

The contents of the book are divided into classifications of tools required for the various phases of life. There is a complete index of six pages.

WE AMERICANS. *A Study of Cleavage in an American City.*

By Elin L. Anderson. Harvard University Press, Cambridge. $3.00. 8 x 5½; xii + 286 + 6 plates; 1937.

Miss Anderson has chosen Burlington, Vermont, for her sociological study of the complex ethnic relationships behind the outward comformities of dress and appearance as seen on active, bustling streets. Burlington is a conservative city with a population of nearly 25,000, in which number are such diverse racial groups as the English, English-Canadians, French-Canadians, Irish, Jews, Italians, Germans, Greeks, and Syrians. This city faces, as do other communities of America, the complex and as yet unsolved problem of ethnic cleavage.

Ethnic distinctions are of prime importance in Burlington, and playing no small part in this segregation is the attitude of the fourth generation Americans, and those who claim even longer ancestral lineage in this country, to the first, second, and even third generation Americans. Each of Burlington's racial groups clings to its own tradition; each has the ethnocentric outlook of the primitive savage, and each is suspicious of the other racial groups. The incorporation of the many racial groups into a unified community of common aims still lies in the future. The story of Burlington indicates how far from realization is "the high dream of creating an American people of all the elements that have been borne into the country on successive waves of immigration."

This study, which has received the third John Anisfield award, profits by its author's sympathetic insight and honest revelation of the racial, religious, social, and economic elements that keep Americans divided into separate ethnic groups.

HARLOW BROOKS *Man and Doctor. Second Edition.*

By John J. Moorhead. Harper and Bros., New York and London. $3.50. 8½ x 5¾; vii + 302 + 9 plates; 1937.

From 1895 until his death, in the forty years in which Harlow Brooks practised his profession in New York City, he not only achieved an important position in the art and science of medicine, but because of genuinely human qualities acquired a host of friends and admirers. The fact that his biography has gone to two editions is a tribute to Brooks's personality and medical reputation as well as to the author's literary skill.

Brooks went directly to New York City after having received his doctor's degree at Michigan and almost immediately he became associated with the Bellevue Hospital Medical College. The author introduces this period of Brooks's life with interesting and often amusing accounts regarding Bellevue Hospital and the leading physicians of the day. From the very beginning, Brooks's uncommon abilities were apparent and he early received due recognition as a pathologist and diagnostician. The high points of his remarkable career including his war services which were almost balked at the start by bureaucratic red-tape are described in a style which is discursive yet not lacking in elegance. The reader is impressed by Brooks's modesty and continuous devotion to the high principles of the art of healing but most of all by the strong affection which he inspired in the author as well as in many others.

SHORT YEARS. *The Life and Letters of John Bruce MacCallum, M.D. 1876–1906.*

By Archibald Malloch. Normandie House, Chicago. $3.50. 9¼ x 6¼; xiii + 343; 1938.

John Bruce MacCallum died two months before reaching his thirtieth birthday. But in that short life he achieved a reputation as an invetigator that has sometimes been compared with Frank Balfour's in respect of brilliancy and promise. Outwardly his short life was a simple and uneventful one—boyhood and undergraduate days in Canada, Johns Hopkins Medical School, a bad summer in Germany, and finally the University of California with Jacques Loeb—but over and through it all the never-ceasing battle against tuberculosis.

Dr. Malloch has done a superb job in the labor of love that made this book. Mac-

Callum's own letters tell the story, but the editing, arranging, and running comment have been managed with consummate artistry by Malloch.

This is a book that every medical student—indeed every student of any branch of science—should read. It will reveal the inherent dignity and beauty of scientific research, and set a spiritual model for living that cannot fail of meaning and helpfulness in a disordered world.

Season of Birth. *Its Relation to Human Abilities.*

By Ellsworth Huntington. John Wiley and Sons, New York. $3.50. $8\frac{1}{2} \times 5\frac{1}{2}$; vii + 473; 1938.

Huntington has studied the seasonal fluctuations in the number of births in this and other countries and concludes that: (1) the kind of weather present at about the time of conception influences the mental and physical condition of the embryo and consequently affects the whole life of the individual; (2) the human race inherits a mechanism sensitive to weather changes; (3) the seasonal oscillations in births are due to the survival of individuals whose seasonal reproductive rhythm coincides with the optimum time for the offspring to be born and live; (4) children conceived and born during the optimum season are more vigorous, stronger, etc., than those born at other periods. It is difficult to judge whether any or all of these conclusions are actually supported by the facts. The author presents his data in graphic form only and consequently it is practically impossible to determine whether the relations observed by him are real or simply apparent. Moreover, there are numerous factors which could be casually responsible for the seasonal fluctuations of births and that the author passes over lightly or fails entirely to take into account. Finally the comparability of the data used is open to question and Huntington's interpretation of differences between countries is often very dubious. Better analytic technique and method is needed before the conclusions advanced by the author can be given serious consideration.

Race. *A Study in Modern Superstition.*

By Jacques Barzun. Harcourt, Brace and Co., New York. $2.50. $8 \times 5\frac{3}{8}$; x + 353; 1937.

In essence, this work is an exposition of the controversies which in France have taken place over the question of race and racial superiority. The author wishes to show that this question brought to the front in Germany today is not an original product of Nazism but that at different periods, in France as well as other countries, it has been the subject of grave and animated discussion by philosophers, writers and scientists. In demonstrating that this belief in "pure" race, racial superiority, etc. is simply a superstition the author tells us nothing new, but in assuming that the disquisitions of the intellectuals are of great weight either in fostering or maintaining this belief he overlooks the most important elements in this phenomenon—the emotional reaction which it arouses in the population group. Moreover, in attempting to show that the question of race has really no scientific meaning the author proceeds to a wholesale indictment and ridicule of the findings of physical anthropology and of the vital and health statistics with regard to "racial" differences. This is a smart journalistic device but leaves doubts as to the author's seriousness of purpose or profundity of knowledge. There is an extensive if not complete bibliography on the subject.

Totemica. *A Supplement to Totemism and Exogamy.*

By Sir James G. Frazer. The Macmillan Co., New York and London. $6.50. $8\frac{1}{2} \times 5\frac{1}{2}$; xii + 518; 1938.

As the sub-title indicates, Frazer here has compiled some of the principal observations concerning totemism made since the publication in 1910 of his monumental treatise. Approximately half of the book is dedicated to a review of the data on Australia. The importance given the observations on the native Australian is due essentially to the fact that more information has been collected about these peoples than about any other and because

from the varied forms in which totemism is manifest among them it is hoped to find a clue to its origin. Besides totemism in Australia, the author reports on the observations made in Melanesia, New Guinea, India, Africa and North America. It is to be noted that in this book more emphasis is placed on the facts than on possible interpretations or generalizations. This is a happy reversal of the method adopted by some of the more enthusiastic of Frazer's disciples. Since the author fails to summarize the observations reported here or comment on their bearing to his earlier conclusions, we hope that in the near future this omission will be remedied by a mature summation and evaluation of the significance of this complex social phenomenon.

NOT SO LONG AGO. *A Chronicle of Medicine and Doctors in Colonial Philadelphia.*
By Cecil K. Drinker. Oxford University Press, New York. \$3.50. $8\frac{1}{2} \times 5\frac{5}{8}$; xii + 183 + 12 plates; 1937.

Professor Drinker here presents extracts and notes from the diary which his great-great-grandmother, Elizabeth Drinker of Philadelphia, kept from 1758 to 1807. The items which he has selected deal in particular with the methods of hygiene and medical practice in vogue in that period. Elizabeth Drinker, it is apparent, was a shrewd and intelligent observer of the life around her. Moreover she took an active interest in medical questions especially when related to the health of her family and was able to avail herself of the best professional care which that period could give. From her diary the reader acquires a vivid picture of the bathing and toilet facilities in the homes of that time and the care and treatment for childbirth, tuberculosis, smallpox, and yellow fever. In addition, there are given interesting pen-portraits of such eminent physicians as Rush, Shippen, Bard, Kuhn and Physick. As noted in the title, the conditions described were present not so long ago, a matter of only 140 years. But this was an interval of progress in public health and sanitation so great that it takes a vivid account like this to give us a true realization of its import.

DR. NASH'S COOKERY BOOK.
By Elwin H. T. Nash. Simpkin, Marshall, London. 6d. net. $8\frac{1}{2} \times 5\frac{1}{2}$; 183; 1937 (paper).

This cook book, issued by an English doctor (whose hobby has long been cooking) furnishes to the family of small income an excellent series of recipes which, if followed with care, will produce palatable dishes with an adequate protein ration at a very low cost. An intelligent and enthusiastic group of workers put in a year's research in experimenting and testing. "Some of these dishes were altered as many as ten times until the right flavour or consistency was obtained. Throughout the researches the price paid for beef averaged 6d. per pound, and for mutton, 3d. per pound." Always it was kept in mind "that the smaller wage earner, speaking generally, likes his flavours more pronounced than other people." If any criticism is to be made, it is that so little space (3 pages only) is devoted to the cooking of vegetables—yet this is not altogether a just criticism for vegetables are used repeatedly in the meat dishes. Being a cookery book, it does not touch upon the many ways in which raw vegetables and fruits can be used. If this reviewer lived in England he would certainly obtain a copy of this book and study it carefully. The chutney recipes are already being tested in our American kitchen.

NOTES ON A DRUM. *Travel Sketches in Guatemala.*
By Joseph H. Jackson. The Macmillan Co., New York. \$3.00. $8 \times 5\frac{1}{2}$; x + 276; 1937.

Because the author's room in Guatemala contained no table but a tall Mayan drum, upon this he wrote his notes on the surrounding country and its people. Hence the title of the book. Mr. Jackson having, like all good tourists, carefully

read up on the country and its history, had fairly definite plans as to what he wanted to see. These he submitted to the Clark Tours people who skillfully and economically arranged his travels in Guatemala. This volume, however, is more than a helpful guide for future travelers to this interesting country, for it is written in an amusing, informal style and is worth reading for entertainment alone. Sixty full page photographs taken by the author enrich the book, giving the reader a more complete idea of the country and its inhabitants. Here and there some of the past history of the country is brought in, making it more interesting for the modern explorer. Unfortunately he has not included a bibliography and there is neither an index or a table of contents.

African Genesis.

By Leo Frobenius and Douglas C. Fox. Stackpole Sons, New York. $3.00. 9 x 6; 236 + 6 plates; 1937.

Because Frobenius' rock picture research led him to believe there might be a transfusion of cultures and faiths between African and Egyptian civilizations, he undertook to investigate the mythology back of these paintings. This body of folklore, the substance of this volume, makes delightful, entertaining and refreshing reading. The legends, collected from a number of African tribes are written in clear straightforward language which is almost poetical in its simplicity of style and idea. The book is illustrated with drawings from rock paintings taken from the different parts of Africa explored by the Frobenius expedition. These have nothing to do with the actual body of the text. However, Fox, in his foréword in which he theorizes about the meaning and origin of these legends, attempts to explain the paintings by the legends. In addition there are a number of extremely well done pencil portraits of the African natives and an occasional map showing the areas in which the rock pictures were collected.

The American Indian. *An Introduction to the Anthropology of the New World. Third Edition.*

By Clark Wissler. Oxford University Press, New York and London. $3.75. $8\frac{1}{2}$ x $5\frac{5}{8}$; xvii + 466 + 9 plates + 1 folding map; 1938.

Dr. Wissler's book can be considered the Bible of New World anthropologists. Starting off with the distribution of the aborigines and their social patterns as determined by the food areas of the continent, he proceeds into the cultural traits of the various tribes. Instead of penetrating any one field thoroughly, the author lightly covers all the phases of culture in a concise manner, but giving sufficient information to enable the reader to gain a conception of the extent to which the Amerinds had progressed. The origin of the American Indian is also discussed. There is appended a classification of the linguistic stocks of the United States and Canada and a bibliography of 32 pages. The tyro or the well-versed scholar will find information, both interesting and new, whether it concerns the Abipones or the Zuni, in this revised edition.

A Bibliography of the Works of Ambroise Paré: *Premier Chirurgien and Conseiller du Roy.*

By Janet Doe. University of Chicago Press, Chicago. $5.00. $10\frac{1}{4}$ x $7\frac{1}{4}$; xx + 266 + 22 plates; 1937.

For the past century Malgaigne's bibliography has served as the authority on the writings of the sixteenth century surgeon, Paré. However, it covers only the editions extant in Paris in 1840, together with those listed by a previous bibliographer, Haller, and contains several mistaken entries copied from previous writers. Miss Doe has now compiled all the available information concerning the editions of Paré's works, including six editions previously unknown to bibliographers, corrected the errors of Malgaigne's list, and furthermore added interesting little digressions into the surgeon's life and personality, his patrons, publishers,

friends and enemies. Thus this book is more than a cut and dried notation of items. Extracts from the books are cited and numerous facsimile pages serve as illustrations.

THE LAND OF THE GURKHAS *or the Himalayan Kingdom of Nepal.*

By W. Brook Northey. With a chapter by the Hon. C. G. Bruce. W. Heffer and Sons, Cambridge. 10s. 6d. net. $8\frac{3}{4} \times 6\frac{1}{8}$; x + 248 + 66 plates + 1 folding map; 1937.

Since there is relatively little information available regarding the kingdom of Nepal and its people, this account written by an officer with 20 years experience in a Gurkha regiment is one that will interest the human biologist and whet his curiosity about this land practically closed to the ordinary traveller. In this volume will be found a comprehensive although necessarily condensed description of the geography of Nepal, of the customs and arts observed in its principal centers and of the history of the Gurkha. The author is apparently not interested either in the ethnology or anthropology of the country and consequently the reader who hopes to obtain a picture of the conditions of life and civilization of Nepal will be disappointed. However, even with these defects, the book will be found interesting and worth delving into for what it gives on the subject.

THE FUTURE OF CIVILISATION AND SOCIAL SCIENCE. *A Study Involving the Principles of Scientific Meliorism.*

By W. P. Dreaper. E. T. Heron and Co., London. 1s. $7\frac{1}{4} \times 5\frac{3}{4}$; 48; 1937 (paper).

Scientific Meliorism, a humanitarian form of socialism, was proposed by Jane Hume Clapperton in 1885 as a means of attaining the ideals of goodness which she believed to be the goal of man's evolution. The theory, as advocated by her, was essentially a general application of the methods so dear to the heart of the Victorian, "to repress in the young certain instincts which are natural and therefore unblamable." In restating and reviving Jane Clapperton's ideas, Mr. Dreaper is apparently unaware that nowadays those interested in the study of society are not receptive to vague theories which are as far from reality as the characters of an Elsie Dinsmore book. It might be added that the 'scientific' in Scientific Meliorism has about the same value as the 'science' in Christian Science.

A HISTORY OF WOMEN IN MEDICINE *From the Earliest Times to the Beginning of the Nineteenth Century.*

By Kate Campbell Hurd-Mead. The Haddam Press, Haddam, Conn. $6.00. 9 x 6; xvi + 569 + 60 plates; 1938.

This is the first of two volumes which together will adequately reveal for the first time the place of women in the history of medicine. This volume carries the story from the earliest times (beginning in Egypt, Assyria, and Palestine), comes down through Greece and Rome to mediaeval Europe, and then includes the various countries of the world up to the beginning of the nineteenth century. The second volume, to be published in the near future, will carry the story up to date. An admirable work is presented here both as to the subject, neglected until now, and as to the diligence in personal research throughout all the different sources in most of the great libraries in Europe. It is indeed a lifetime study that began forty years ago. Every page is interestingly and fascinatingly written and numerous quotations from the original sources are included. Naturally it will be of great interest to the historian of medicine. The index covers more than fifty pages.

MEDICAL CLASSICS. *Volume I, Numbers 2 to 10.*

Compiled by Emerson Crosby Kelly. Williams & Wilkins Co., Baltimore. Subscription price $10.00 per volume; single copies $1.25. 10 x 7; 81–928; 1936–1937 (paper).

These numbers complete Vol. 1 of this

series of publications. Similar to the first number (cf. Q. R. B., Vol. 12, p. 240), each of these presents a brief biographical sketch of the author whose work is extracted or reprinted, bibliographic data, and a note regarding the influence and significance of the work given. Each of the numbers contains reprints of one or more articles or selections from the writings of one of the following authors: Charles Bell, Oliver Wendell Holmes, Percivall Pott, Theobald Smith, Dominic J. Corrigan, Thomas Hodgkin, Nathan Smith, Douglas Argyll Robertson and Guido Banti. As the reader will note, the names of the majority of these authors are associated with disease entities regarding the clinical description of which precious little has been added in recent years. It seems almost superfluous to say that a careful reading of these papers will interest and benefit not only the physician flut also the biologist.

Fifth Avenue to Farm. *A Biological Approach to the Problem of the Survival of Our Civilization.*

By Frank Fritts and Ralph W. Gwinn. Harper and Bros., New York and London. \$3.00. $8\frac{1}{4} \times 5\frac{5}{8}$; viii + 282; 1938.

Belief that American civilization is becoming genetically inferior, due to migration of the superior individuals from farms to cities, is the theme of this book. These individuals then fail to reproduce themselves, while the inferior persons left on the farms are the reproducers of the population. The proposal is made that country life could be very attractive to the biologically more desirable elements, and that a back to the farm movement should be stimulated. Granted that the country is, or could be, an attractive place for anyone to live, nevertheless the authors are treading on thin ice in assuming superiority or inferiority and its transmission in such extensive groups as urban and rural populations.

The Marginal Man. *A Study of Personality and Culture Conflict.*

By Everett V. Stonequist. Charles Scribner's Sons, New York. \$1.60. $7\frac{3}{4} \times 5\frac{3}{8}$; xviii + 228; 1937.

By marginal man is here meant the individual who either through heritage or migration is brought into close contact with two different and often antagonistic types of civilizations and cannot participate completely in either. Thus the author examines the problems of the descendants of some of the race (color) marriage mixtures of India, South Africa, United States, Java, Hawaii and Brazil. He discusses also the problems and methods of adjustments of the European immigrants in the United States and of their offspring, and of the so-called racial minorities in Europe. As a source of bibliographic references this book will be found useful, but otherwise it is a somewhat diffuse compilation that demonstrates very little originality except in chapter and section headings.

Rassenkunde und Rassengeschichte der Menschheit. *Zweite umgearbeitete und erweiterte Auflage in zwei Bänden. Erster Band. Die Forschung am Menschen. Erste Lieferung (Bogen 1–8). Zweite Lieferung (Bogen 9–16). Dritte Lieferung (Bogen 17–22).*

By Egon F. von Eickstedt. Ferdinand Enke Verlag, Stuttgart. Bogen 1–8, RM. 8.; Bogen 9–16, RM. 8.; Bogen 17–22, RM. 6.60 (25 per cent less outside of Germany). $10\frac{1}{4} \times 7$; 352; 1937–38 (paper).

The first edition of this monumental history of the races of mankind appeared in 1934. The necessity for a new edition after only three years is a good sign of the significance and popularity of the work. In the three numbers now at hand an introduction into the fundamentals of anthropology and the history of research on man since antiquity is given in connection with numerous neighboring points from biology, geography, history, and the arts. The racial standpoint of modern Germany and the Nordic idealogy of Gobineau and Chamberlain receive relatively brief treatment in comparison with the broad quotations from other literature. Each number is extensively documented.

FAMILY LIVING IN KNOTT COUNTY, KY. *U. S. Department of Agriculture. Technical Bulletin No. 576.*
By Faith M. Williams, Hazel K. Stiebeling, Idella G. Swisher and Gertrude S. Weiss. Government Printing Office, Washington. 10 cents. $9\frac{1}{4} \times 5\frac{7}{8}$; 69 + 1 plate; 1937 (paper).
This study is part of a comprehensive survey of economic and social conditions in the southern Appalachians, and attempts to show the effect of farming upon material conditions of family life. It was found that the level of living of the 228 families studied was, on the whole, lower than that of many groups of farm families in other areas. The authors conclude that the problems of the people of this section call for economic action on a greater scale than the individual family can undertake. There is a bibliography and an appendix containing a classification of the goods and services entering into the value of current family living.

PRELIMINARY REPORT ON THE SMITHSONIAN INSTITUTION-HARVARD UNIVERSITY ARCHEOLOGICAL EXPEDITION TO NORTHWESTERN HONDURAS, 1936. *Smithsonian Miscellaneous Collections, Vol. 97, No. 1. (Publication 3445).*
By William D. Strong, Alfred Kidder II and A. J. Drexel Paul, Jr. Smithsonian Institution, Washington. 80 cents. $9\frac{3}{4} \times 6\frac{3}{8}$; v + 129 + 16 plates; 1938 (paper).
This progress report on excavations of mounds in the valley of the Ulua River, in 1936, describes fragments of pottery made by Maya, Jacaque, and Lenca peoples in pre-Columbian times and discusses their usefulness in tracing cultural trends. Potsherds and reconstructed vessels and figurines are described in considerable detail and illustrated by a large number of drawings and photographs. Some descriptions of the inhabitants of the region, written by early Spanish explorers and colonists, are presented in translation.

PHYSICIANS OF THE MAYO CLINIC AND THE MAYO FOUNDATION.
Division of Publications, The Mayo Clinic. University of Minnesota Press, Minneapolis; Oxford University Press, London. \$10.00. $9\frac{1}{4} \times 6\frac{1}{4}$; vi + 1575; 1937.
This volume is divided into four sections: (1) a main section containing biographies, in alphabetic order, and bibliographies, in chronological order, of persons who served in the Mayo Clinic or Foundation for one year or more; (2) a section containing brief biographies of persons who were officially connected with the Clinic or Foundation for less than a year; (3) an index of universities or colleges from which persons appearing in the main section received their degrees; (4) a geographic index of the scientific workers from all over the world who have been connected with the Rochester clinic.

THE HERVEY ISLANDS ADZES *in the Peabody Museum of Salem.*
By Ernest S. Dodge. Peabody Museum, Salem, Mass. 75 cents. $10 \times 7\frac{1}{4}$; iv + 16 + 13 plates; 1937 (paper).
This is the second of a series of catalogues to be published by the Peabody Museum of Salem on its collection of Polynesian ethnological material. The present publication contains illustrations and descriptions of thirty-two specimens of a single type of object—the adz—an instrument of questionable purpose, consisting of an axlike blade set in a handle which is often ponderous and usually elaborately carved.

BERICHT DER 12. VERSAMMLUNG DER INTERNATIONALEN FEDERATION EUGENISCHER ORGANISATIONEN. *Konferenzsitzungen vom 15. bis 20. Juli 1936. Scheveningen, Holland.*
W. P. Van Stockum and Zn. N.V., The Hague, Holland. Flor. 3. $9\frac{3}{4} \times 6\frac{1}{2}$; 119; 1937 (paper).
This volume of proceedings contains 25 papers, published either *in toto* or in abstract, on race hygiene, sterilization laws, mutation and other topics of interest to eugenists. Two of the papers are in French, the others, about equally divided, in English or German. The articles are preceded by a list of members of the International Federation of Eugenic

Organizations, and followed by reports of committees.

Handbuch der Biologischen Arbeitsmethoden. *Lieferung 468. Abt. VII, Methoden der vergleichenden morphologischen Forschung, Teil 2, Heft 3.* Containing the following article: *Spezielle Methoden anthropologischer Messung,* by Theodor Mollison.
Urban and Schwarzenberg, Berlin. RM. 9. 10 x 7; 160; 1938 (paper).

This is a description of the most important methods of anthropological measurements on the living subject and on the skeleton, including instruments and graphical representation. It concludes with a brief statistical treatment of results. It is provided with numerous illustrations and a bibliography.

A Graphic Summary of the Number, Size, and Type of Farm, and Value of Productions (*Based Largely on the Census of 1930 and 1935*). *U. S. Department of Agriculture, Miscellaneous Publication No. 266.*
By O. E. Baker. Government Printing Office, Washington. 10 cents. $9\frac{1}{4}$ x $5\frac{7}{8}$; 76; 1937 (paper).

For a brief bird's-eye view of the status of agriculture in the United States today, this bulletin has been excellently prepared. The increase or decrease in number and size of the different types of farms, together with the value of products are shown graphically by map diagrams. The data are taken largely from the census reports of 1930 and 1935.

ZOÖLOGY

The Birds of America.
By John James Audubon. With an Introduction and Descriptive Text by William Vogt. The Macmillan Co., New York. \$12.50. $12\frac{1}{4}$ x 9; xxvi + 500 plates; 1937.

The reputation of this classical work renders any extensive detailed comment unnecessary. The current edition commemorates the centennial anniversary of the original publication, and contains sixty-five additional illustrations of birds from the Pacific slope, which Audubon painted at a later date. The original edition of *The Birds of America* was published in London in the years 1827–38. Each plate was reproduced from an original water-color painting by Audubon. The process used for reproduction was aquatint engraving on copper plates, with hand coloring of the impressions. The plates were printed on Whatman paper of double elephant folio size $39\frac{1}{2}$ x $26\frac{1}{2}$ inches before trimming. There were 435 of these plates. The set was priced at £ 182/14 in England, and \$1000 in the United States. In 1840–44 an octavo edition (lithographed and hand-colored) in seven volumes was published in New York and Philadelphia.

The present work is the first edition of these famous plates ever to be issued in a single volume. And it is a superb piece of book-making. The plates are reproduced by lithography and on the whole extraordinarily well. At the price the book is a real bargain.

The brief texts accompanying each plate, while necessarily very condensed have been extremely well done by William Vogt. Ranges, habitats, and conspicuous diagnostic characters are indicated.

Altogether we congratulate the publishers on the fine service they have done biology in producing this book.

The Call of the Koala.
By Ambrose Pratt. Robertson and Mullens, Melbourne. 6s. $8\frac{1}{2}$ x $5\frac{1}{2}$; 120 + 18 plates; 1937.

Koala. *The story of Australia's Native Bear.*
By Charles Barrett. Robertson and Mullens, Melbourne. 2s. $8\frac{1}{2}$ x $5\frac{1}{2}$; 31 + 15 plates; 1937 (paper).

When we take cognizance of the fact that in the early twenties of the present century as many as 1,500,000 Koala pelts were exported from Australia to Britain alone per annum, and that today the entire

race of Koalas numbers less than 1,000, we realize and appreciate how nearly this "native bear" came to the fate of our own American passenger pigeon—extinction. It is for the purpose of conservation, and appreciation of what this popular little animal has to offer to the world of science as well as to the entire nature-loving world, that these two little books have been written.

The fourteen years of study which have led the author of the first volume into every phase of the Koala's life have made him well qualified to write on the subject. Pratt's work has included detailed studies of the anatomy, physiology, classification, origin, and food of Australia's "native bear." Particularly has he been interested in determining the cause and discovering a method of preventing the high death rate among bears both in captivity and in the free state. The work has presented almost unsurmountable difficulties in that (1) it is pioneer in nature, and (2) it deals with an organism that is almost entirely incapable of adapting itself to conditions outside the narrow realm in which it has lived practically unchanged for the past million years. This little volume is truly a scientific treatise, written in a popular style, and with a note of sympathy that appeals to every nature lover.

The second volume deals with the early history of the Koala, its classification, habits and value, together with the effect of colonization of its native Australia on its natural population.

Both books include a number of excellent photographs of the Koala in some of its most characteristic poses. The former includes several photos of the different species of *Eucalyptus*, the leaves of which make up the entire diet of this strange little marsupial.

Silver Fox Pelt Prices as Affected by Time of Pelting, Sex, and Age. *U. S. Department of Agriculture, Circular No. 460.*
By Chas. E. Kellogg. Government Printing Office, Washington. 10 cents. $9\frac{1}{4}$ x 6; 27 + 2 plates; 1937 (paper).

The industry of silver fox fur farming is of such recent origin that factual information concerning its many phases has not yet been collected to any great extent. Statistics concerning the effect of time of pelting, sex, age, degree of silver, etc., on the value of pelts have been needed for the obvious purpose of putting the industry on a more scientific and profitable basis.

The Bureau of Biological Survey of the Department of Agriculture has made a study of pelt values based on the industry of the Herbert A. Nieman Co., Thiensville, Wis., and has published the findings in this bulletin. The data concerning the value of the furs were collected by tabulating the prices received for some 10,000 pelts sold by the New York Auction Co. in November, December, January, and February of 1935–36. In calculating the mean value for furs of different types sold each week during the period, it is assume that the quality of the pelt alone determines the price received, and no mention is made of the possible effect of supply and demand on the variability of price.

The Food of North Sea Herring 1930–1934. *Fishery Investigations, Series II. Vol. XV. No. 5.*
By R. E. Savage. His Majesty's Stationery Office, London; British Library of Information, 270 Madison Ave., New York. 95 cents. $10\frac{3}{4}$ x $7\frac{1}{4}$; 57; 1937 (paper).

Marine Fauna of the Isle of Man. *Reprinted from "Proceedings and Transactions of the Liverpool Biological Society," Volume 50.*
Compiled by Hilary B. Moore. Edited by R. J. Daniel assisted by J. R. Bruce and M. W. Parke. University Press of Liverpool, Liverpool. 2s. net. $8\frac{1}{2}$ x $6\frac{1}{4}$; 293; 1937 (paper).

Two selected areas of the North Sea were used as collecting grounds for the 34,124 herring whose stomach contents were examined in this study. The principal organisms present in the food of the herring and their prevalence and sequence during stated intervals of time were noted, the percentage composition of the food calculated and the total volume of the food ascertained. Comparisons were made

of annual variations in the quantity and type of food consumed and comparative estimates made to show the relative quantity of food taken out of the water each season. Twelve reference works are listed in the bibliography.

Old and new data have been brought together in *Marine Fauna of the Isle of Man* in the preparation of a classified list of the marine animals found on the shores of this island between tide marks, and in the surrounding sea to a distance of about five miles. An up-to-date record of the distribution of these forms is included. Three charts show the location of the various collecting grounds, all of which are briefly described in the text with the exception of the Dub Reef of Port Erin Bay for which a detailed account of the fauna is given. The book includes an index and both reference and record bibliographies.

GIANT FISHES, WHALES AND DOLPHINS.

By J. R. Norman and F. C. Fraser. Illustrated by Lieut. Col. W. P. C. Tenison. W. W. Norton and Co., New York. $4.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; xxvii + 341 + 8 plates; 1938.

This book is the scientific reply to the many inquiring sportsmen, travelers, and seafaring men who, for sundry reasons, desire information concerning whales, dolphins, and larger kinds of fish. To merit a place in this work Neptunian members must possess a physical combination of true backbone and six feet of length. The only ones to qualify were the six-foot sharks, rays, bony fishes, whales, dolphins, and porpoises. The lampreys and hagfishes were omitted, and such enormously long members of the Coelenterata, Mollusca, and Arthropoda as are popularly called "fishes" were also excluded for lack of a qualifying backbone.

The book is divided into two parts. Part I deals with the Selachii and Pisces; Part II, with the Cetaceans. Conversationally written in generally nontechnical language and arranged for facile, ready reference, this work is both a scientific and a picturesque presentation of its subject. History, legend, and personal anecdotes pleasantly flavor scientific descriptions of the breeding habits, diet, habitat, structure and appearance of the selected creatures. Simple keys for identification are also included. There are 8 colored plates, 97 drawings for identification purposes, and other numerous quaint historical and legendary sketches.

ATLAS OF THE SCALE INSECTS OF NORTH AMERICA.

By G. F. Ferris. Stanford University Press, Stanford University; Oxford University Press, London. $8.75. $10\frac{3}{4}$ x $8\frac{1}{2}$; 8 + [544]; 1937.

The ambitious purpose of this series (of which this is only the first part) is "to make possible the definite identification of every species of scale insect now known from North America." To make this project feasible the book is in atlas form and is essentially a collection of plates of the different species with a minimum of textual material. One can grasp the magnitude of the problem from the fact that this first series covers only the Tribe Diaspidini of the Family Diaspididae; the latter is only one of the 11 families in the Coccoidea. The scale insects are a confusing group, and at the same time their importance to man is so great that this monograph is doubly welcome. Economic entomologists will find this series indispensable.

GREEK WOLF-LORE. *A Dissertation in Greek Presented to the Faculty of the Graduate School of the University of Pennsylvania in Partial Fulfillment of the Requirements for the Degree of Doctor of Philosophy.*

By Richard P. Eckels. (Small number of copies for free distribution by the author, 1 N. Harrisburg St., Steelton, Pa.) $9\frac{1}{4}$ x $6\frac{1}{8}$ inches; 88, 1937 (paper).

A doctoral dissertation setting forth and discussing what the ancients, particularly the Greeks, knew or did not know about the wolf; beliefs and superstitions about him and something of their later history; the werewolf and 'lycanthropy' (medical); the place of the wolf in certain Greek

cults; and the wolf as nurse, with special reference to the she-wolf who suckled Romulus and Remus.

The significant point is that, where the ancients had numerous superstitions and fallacious ideas about wolves, we moderns have only a few (e.g., that wolves run in packs and that they attack men out of hunger or even out of malice) and those few quite as erroneous and as staunchly believed in as those held as articles of faith in bygone days.

Indices of the source material and of the authors cited and referred to are provided. The treatise will interest not only classicists and folklorists but also historians of science and of medicine.

A MONOGRAPH OF THE ACANTHODRILINE EARTHWORMS OF SOUTH AFRICA.

By Grace E. Pickford. W. Heffer and Sons, Cambridge. 25s. net. $9\frac{3}{4} \times 6$; 612 + 2 folding charts; 1937.

A good many years went into the making of this comprehensive survey of the Acanthodrilinae of South Africa. Where they had already been studied (viz., scattered localities in the region of Port Elizabeth, Cape Peninsula and the Knysna Forest, and a few places in Natal and the Transvaal) they exhibited a high degree of local endemicity.

It was the purpose of the present investigation to study the unknown intervening regions with a view to elucidating this problem of local endemicity in the light of past and present geographical and climatic conditions. It is hoped that the preliminary results so obtained may form the basis for some more complete survey by future investigators.

A valuable reference work. It is abundantly illustrated and has an appendix of taxonomic notes on non-South African genera and species re-examined in connection with the present investigation, and an alphabetical index to the South African species of Acanthodrilinae. Unfortunately there is no general index.

A NATURE LOVER IN BRITISH COLUMBIA.

By H. J. Parham. H. F. and G. Witherby, London. 8s. 6d. net. $8\frac{1}{2} \times 5\frac{1}{2}$; 292 + 21 plates; 1937.

Soon after the author settled in southern British Columbia, he began to observe the wild life about him and before long knew most of the birds, mammals, and fish of the region. In a pleasantly rambling fashion he relates experiences that show keen observational powers. He is greatly vexed by the lack of protection afforded the wild life by the government and he devotes an entire chapter to the methods by which the birds might be saved through governmental aid. The author unfortunately digresses in the last chapter entitled "The search for truth," wherein he expounds his philosophy of life. Otherwise the book is very entertaining and enjoyable to read. There is an index and several appendices that list the mammals, birds, fish, and plants of the Okanagan Valley with their scientific names.

THE BIOLOGICAL CONTROL OF AN INSECT IN FIJI. *An Account of the Coconut Leaf-mining Beetle and its Parasite Complex.*

By T. H. C. Taylor. Imperial Institute of Entomology, London. 12s. net. $9\frac{3}{4} \times 6\frac{3}{8}$; 239 + 23 plates; 1937.

This smoothly-written account of the successful control of the leaf-mining beetle *Promecotheca reichei* Baly in Fiji by biological means begins with an account of the life history of the pest and of its principal parasites. Oddly enough, one of its parasites, the mite *Pediculoides ventricosus* Newp., complicated the problem by "periodic elimination of all developmental stages, except one, of *P. reichei* itself," thus depriving the other parasites of one of their principal sources of food for a large part of the year. The practical details of selecting and rearing parasites and estimating their effect on the pest are described and the principles of biological control are concisely stated. The book is well illustrated with photographs and drawings of the insects discussed. There is no index.

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society, Volume XXII, Part 4, Numbers 21–28.*

New York Zoological Society. Zoological

Park, New York. \$1.80. $10\frac{1}{2} \times 7$; 111 + 3 plates; 1937 (paper).

In this issue of *Zoologica* the first paper is on the histological structure of the normal and the hyperplastic thyroid in *Rasbora lateristriata* (Bleeker), a small tropical fish. Serial sections (3 plates) show that the excessive growth of the thyroid destroys and replaces the gill structures. The other papers are on lymphocystis in the hogfish *Lachnolaimus maximus*, on the "display" of the Twelve-wired Bird of Paradise and of the Magnificent Rifle Bird, on decapod crustacea from the Lower California region, on deep-sea fishes of the Bermuda oceanographic expedition [Serrivomeridae (genus *Platuronides*) and Nemichthyidae], and the caudal skeleton of Bermuda shallow water fishes (order Iniomi: Synodontidae). All of the papers have text figures.

HORMONE BEI WIRBELLOSEN TIEREN. *Probleme der Biologie, Band I.*

By Gottfried Koller. Akademische Verlagsgesellschaft M.B.H., Leipzig. RM. 9.20 (paper); RM. 10.80 (cloth). $8\frac{1}{4} \times 5\frac{3}{4}$; viii + 143; 1938.

This little book is a summary of all that is known on the subject of hormones among invertebrates. The author in defining his concept of hormones points out that recent work has shown that there are many stuffs in organisms that have peculiarities in common with hormones but cannot be said to be glandular products. These stuffs are discussed under the following chapter headings: Cell hormones; Aglandular tissue hormones; Glandular tissue hormones; Hormones producing color changes; Glands of internal secretion among molluscs and tunicates.

BATTLES WITH MONSTERS OF THE SEA.

By F. A. Mitchell-Hedges. D. Appleton-Century Co., New York. \$4.00. $8\frac{3}{4} \times 6\frac{1}{8}$; x + 349 + 24 plates; 1937.

This is a lively narrative of fishing thrills off the coast of British Honduras and among the islands off the coast of the Republic of Honduras and in the Gulf of Panama. Although the author was collecting all the time, this is mainly a story of adventure, excitement and hair-breadth escapes. The captures include a 960 pound jewfish, a 1,960 pound tiger shark, a 4,600 pound saw-fish and an eagle-ray weighing 5,200 pounds. For those interested in adventure and the endless bounties of southern waters and tropical islands, this book with its pleasant style should bring great enjoyment. Many clear photographs accompany the text and there is a complete index.

LIFE OF THE OCEAN (FISHES, ETC.). *Wonders of the Sea. Art and Nature in Colour Series.*

Introduction by E. G. Boulenger and A. Portmann. Plates painted from nature by Paul A. Robert. B. T. Batsford, London. 7s. 6d. net. $13\frac{3}{4} \times 10\frac{1}{8}$; 8 + 12 plates; 1938 (paper).

This volume is another number in a series dealing with "Art and Nature in Colour," which has already been noticed in these columns. The compilers have gone to the bottom of the sea for their material. The beautifully colored illustrations are replete with forms of animal and vegetable life which quickly lose their color when removed from their natural environment, and with which consequently, the majority of people have no first hand acquaintance. The book is more likely to appeal to the artist than to the scientist.

WORLD NATURAL HISTORY.

By E. G. Boulenger. With an Introduction by H. G. Wells. Charles Scribner's Sons, New York; B. T. Batsford, London. \$3.00. $8\frac{1}{2} \times 5\frac{1}{2}$; xx + 268 + 150 plates; 1938.

While extremely condensed—so that the Hymenoptera for example get only a trifle over two pages—it seems fair to say that for its size this is the best general natural history (dealing only with animals) yet to appear. The illustrations are superb. Anyone wanting to put in the hands of his children a general conspectus of animal life cannot do better than to give them this

book. Incidentally they will learn a lot about evolution.

AXIAL BIFURCATION IN SERPENTS. *An Historical Survey of Serpent Monsters Having Part of the Axial Skeleton Duplicated.*
By Bert Cunningham. Duke University Press, Durham, N. C. $2.50. 9 x $6\frac{1}{8}$; vii + 117; 1937.

For anyone interested in ophidian teratology, this book will furnish extensive information. It cites all records that have ever been reported, including historical accounts, on dichotomy in serpents, and whenever possible gives an illustration of the specimen.

There is a bibliography, indices of authors and specimens, and 12 plates consisting of photographs and X-rays of recent material, and photographic copies of the earlier reproductions.

SWIFT MOVEMENT IN THE TREES *and at Their Roots.*
By Phyllis Kelway. Longmans, Green and Co., New York. $2.50. $7\frac{7}{8}$ x $5\frac{5}{8}$; x + 189 + 15 plates; 1937.

These are stories of Miss Kelway's charmingly intimate experiences with her animal friends including squirrels both red and grey, shrews, moorhens, and her pet toads, Geraldine, Boadicaea, and the lively Natterjack. Although the stories themselves are of little scientific interest, her numerous photographs of these wild creatures are excellent. Her way of interpreting their feelings, their psychology and their language is nothing short of amazing.

THE OXYSTOMATOUS AND ALLIED CRABS OF AMERICA. *United States National Museum Bulletin* 166.
By Mary J. Rathbun. Government Printing Office, Washington. 60 cents. $9\frac{5}{8}$ x $6\frac{1}{4}$; vi + 278 + 86 plates; 1937 (paper).

This publication is a continuation of Dr. Rathbun's work on the crabs of America. It is the fourth of the series and it maintains the fine standards of workmanship that characterized the previous volumes. Each species is described, the range and measurements given, and usually a table of the material examined by the author is included. There are many pen and ink drawings and 86 plates of photographs.

AFRICAN HUNTER.
By Bror von Blixen-Finecke. Translated from the Swedish by F. H. Lyon. Alfred A. Knopf, New York. $2.75. $7\frac{1}{2}$ x $5\frac{1}{2}$; [8] + 284 + viii + 22 plates; 1938.

Game laws and licenses, characteristics of the natives, and wild animal traits all interest one in this book more than the courageous deeds of the author or his associations with the elite. Changes in the characteristics and supervision of Africa during the past twenty-five years are well depicted. The volume is indexed and contains many good photographs.

THE SNAKES OF NEW JERSEY, *A Guide.*
By Harold Trapido. The Newark Museum, Newark, N. J. 60 cents. $9\frac{1}{8}$ x 6; 60; 1937 (paper).

This is a guide to the 21 species of snakes known to occur in New Jersey. There is a key to the species, each of which is illustrated by a photograph, with notes on the range, occurrence in New Jersey, habitats, and habits. The pamphlet will be extremely useful to natural history students.

DIE BLATT-MINEN MITTEL- UND NORD-EUROPAS EINSCHLIESSLICH ENGLANDS. *Bestimmungs-Tabellen aller von Insekten-Larven der verschiedenen Ordnungen erzeugten Minen. Lieferung 6.*
By Martin Hering. Gustav Feller, Neubrandenburg. Subscription price for 6 numbers: (Germany and Switzerland) 12 marks; (foreign, except Switzerland) 9 marks. $9\frac{1}{4}$ x $6\frac{1}{4}$; 561–631; 1937 (paper).

The concluding number of an alphabetized list of plants attacked by leaf-miners. This contains additions to the list of

plants, correction of errors, classification of the plants and insects considered, and an index of these insects. Preceding issues are reviewed in volumes 11 and 12 of Q. R. B.

Third List of Antillean Reptiles and Amphibians. *Bulletin of the Museum of Comparative Zoölogy at Harvard College. Volume LXXXII, No. 2.*
By Thomas Barbour. Museum of Comparative Zoology, Cambridge. 75 cents. 9½ x 6¼; 92; 1937 (paper).

Recent work has necessitated the publication of a third list of the Antillean reptiles and amphibians. The forms listed from this region now number 483.

A Statistical Study of the Rattlesnakes. *IV. The Growth of the Rattlesnake. Occasional Papers, San Diego Society of Natural History, No. 3.*
By Laurence M. Klauber. San Diego Society of Natural History, San Diego, Cal. 8½ x 5½; 56; 1937 (paper).

The data used in the analysis comprise body lengths and weights, growth measurements, number in brood, snake den populations, etc. Tables and graphs have been included.

A New Snake of the Genus Sonora from Mexico. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 27.*
By Laurence M. Klauber. Society of Natural History, San Diego, Calif. 10⅛ x 6⅞; 4; 1937 (paper).

A New Sea-Urchin from the "Oligocene" of Oregon. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 28.*
By Hubert L. Clark. Society of Natural History, San Diego, Calif. 10⅛ x 6⅞; 8 + 1 plate; 1937 (paper).

An Extinct Puffin from the Pliocene of San Diego, California. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 29.*
By Loye Miller. Society of Natural History, San Diego, Calif. 10⅛ x 6⅞; 4; 1937 (paper).

An Upper Pleistocene Fauna from the Baldwin Hills, Los Angeles County, California. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 30.*
By George Willett. Society of Natural History, San Diego, Calif. 10⅛ x 6⅞; 28 + 2 plates; 1937 (paper).

Notes on the Development of Two Young Blue Jays (Cyanocitta cristata).
By A. L. Rand. Proceedings of the Linnaean Society of New York, No. 48: 27–58, 1936 (1937).

BOTANY

Range Plant Handbook.
Prepared by Forest Service, U. S. Department of Agriculture. Government Printing Office, Washington. $2.50. 9 x 6; iv + 841 + xxvi; 1937.

This book is not available for general distribution, but may be purchased in bound form from the Superintendent of Documents in Washington, D. C. It is an excellent source for identifications and uses of range plants. The 294 illustrations, which were nearly all made especially for this handbook, are more than adequate, both aesthetically and practically. For purposes of identification the drawings and photographs are in minute detail and fully labeled. The 339 text accounts include nearly 900 range species and vacant spaces are provided for later insertions. The field edition is in looseleaf form and a convenient symbol sequence is used in place of numeral pagination. Also, the index may be used as a checklist of preferred names for outstanding range plants. Though prepared for government officials whose duties lie in the management of range lands, this handbook should also be in the botanist's library.

Florida Wild Flowers. *An Introduction to the Florida Flora. Second Edition.*
By Mary Francis Baker. The Macmillan

Co., New York. \$3.50. $7\frac{1}{2} \times 5$; xiii + 245 + 48 plates; 1938.

From the many flowering plants of Florida the author has selected 800 herbs, shrubs, and trees for this edition (enlarged and more profusely illustrated than the earlier one) and in simple terms has indicated their individual characteristics. There is an excellent key in which the plants, with notations on their size and shape, are classified under color headings. The key also acts as an index and refers the reader to the text where, under family headings, further details are given for the identification tentatively established in the key. There is a general alphabetical index in addition to the key.

De l'Influence de divers Cations sur le Croît microbien. *Étude de l'Action a Doses Diverses du Lanthane, du Cérium, du Plomb, du Mercure, de l'Argent, sur la Multiplication du Bacille Pyocyanique. Rôle des Charges Électriques.*

By Lucien Neipp. Masson and Cie, Paris. 90 francs. $10 \times 6\frac{1}{2}$; 430 + 5 folding charts; 1937 (paper).

For this important work the author has studied quantitatively the influence of certain cations on the growth of bacterial colonies. After extensive study of the literature and experimentation he concludes that with the exception of silver, which gave an antiseptic action, all the cations utilized (lanthanum, cerium, lead, and mercury) accelerated the growth when given in small dosages but inhibited it when the doses were large. There appears to be no relation between the value of the electric charge and normal bacterial multiplication, nor can the acceleration or inhibition be attributed to a variation in the electrification of the germs. A bibliography includes 528 titles.

The Structure and Development of the Fungi. *Second Edition.*

By H. C. I. Gwynne-Vaughan and B. Barnes. The University Press, Cambridge; The Macmillan Co., New York. \$5.50. $8\frac{1}{2} \times 5\frac{5}{8}$; xvi + 449; 1938.

The second edition follows almost exactly the sequence of the contents in the first edition (Q. R. B., vol. 3, no. 2) with the exception that the section *Forms Resembling Fungi*, is placed in a more appropriate position in the later edition. As before, the new edition deals with the morphology and physiology of fungi and with mycological technique. New advances in the investigation of the fungi are included. There are also additional illustrations, though most of the older illustrations from the first edition have been retained. The bibliography is more extensive than that of the former edition.

Gardening Indoors. *The Enjoyment of Living Flowers and Plants the Year Round, and New Opportunities for Home Decoration.*

By F. F. Rockwell and Esther C. Grayson. Line Drawings by Esther C. Grayson and Laurence Blair. The Macmillan Co., New York. \$2.50. $8\frac{1}{2} \times 5\frac{3}{4}$; xvi + 201 + 39 plates; 1938.

All of the practical and artistic phases of indoor gardening are covered in this book in a thoroughly adequate manner. With the more general use of air conditioning in homes and apartments, and the use of growing plants as integral parts of modern interior decoration, the newer methods suggested by these well-known authors are timely and valuable. The photographs, charts and classifications of plants for seasonal and special purposes, and a well-organized index make this book an excellent example of a handy volume.

Guide to Eastern Ferns.

By Edgar T. Wherry. Illustrated with line drawings by Olive Stoner and Cyrus Feldman. Science Press Printing Co., Lancaster, Pa. \$1.00. $6\frac{1}{2} \times 4$; iv + 220; 1937.

The smallness of the book, the sizableness of the print, and the illustrated descriptive identifications of ferns and fern-allies make this a convenient field aid for the region covered—Pennsylvania and New Jersey to Virginia. Besides an alphabet-

ical glossary of technical terms, there are brief notes on suitable soils for fern cultivation and on the growing of ferns from spores.

MORPHOLOGY

THE NATURAL HISTORY OF THE FRILLED SHARK CHLAMYDOSELACHUS ANGUINEUS. *The Bashford Dean Memorial Volume. Archaic Fishes. Article V.*

By Eugene W. Gudger and Bertram G. Smith. American Museum of Natural History, New York. \$1.00. $12\frac{1}{4} \times 9\frac{1}{2}$; 243–330 + 5 plates; 1933 (paper).

THE ANATOMY OF THE FRILLED SHARK CHLAMYDOSELACHUS ANGUINEUS GARMAN. *The Bashford Dean Memorial Volume. Archaic Fishes. Article VI.*

By Bertram G. Smith. American Museum of Natural History, New York. \$3.50. $12\frac{1}{4} \times 9\frac{1}{2}$; 331–520 + 7 plates; 1937 (paper).

The phylogenetic position of *Chlamydoselachus* caused much controversy in its day, and while its exact affinities are still in doubt, there is no question that it occupies a primitive position among the vertebrates. Since it was first described by Garman in 1884, there has been little information published about it. One reason for this dearth of material lies in the fact that this shark has rarely been taken outside of Japanese waters. The authors have gathered all known data, using Dr. Dean's notes and available material, and synthesized them into this book.

The second volume is a detailed anatomical description of the frilled shark in which the author has "endeavored to distinguish those features that represent a high degree of differentiation, from others that link *Chlamydoselachus* with the most primitive fishes." The author concludes that the frilled shark "presents a strange assemblage of characters ranging from the very primitive to highly differentiated. *Chlamydoselachus* is a deep-sea adaptation of some rather ancient type of shark, and is now waging a losing battle in the struggle for existence."

Both volumes are excellently prepared and profusely illustrated. This rare and interesting shark has now received some of the attention it so well deserves.

A MONOGRAPH ON VEINS.

By Kenneth J. Franklin. Charles C Thomas, Springfield, Ill. \$6.00. 9 x 6; xxii + 410; 1937.

In this book the writer

has attempted, however imperfectly, to show what is permanent in our knowledge of veins, to indicate where additions of value are even now being made to it, and where opinions about certain aspects of it are changing in the light of fresh research. The review is primarily intended for those engaging in such research, though it should also be of use to others, who wish to know more about the veins and the important parts they play in the harmonious interactions within the living organism. It has been "possible to formulate from a very scattered literature a reasonably consecutive and integrated story. Indeed, nothing has surprised the writer more than the way in which the individual pieces have fitted together."

A work of first rank. The bibliography runs to 27 printed pages and there is an adequate index.

LEITFADEN DER ENTWICKLUNGSGESCHICHTE DES MENSCHEN.

By Horst Boenig. Georg Thieme Verlag, Leipzig. RM. 10.20 (paper); RM. 11.80 (bound); (Outside of Germany, except Palestine, 25 percent less). 10 x 7; x + 266; 1938.

This textbook of human embryology has been very well illustrated both with actual photographs and many diagrams. It is clearly written and includes discussions of the most recent discoveries in the field. The author starts with an account of the development of the sperm and egg, then proceeds to an account of the fertilized ovum, placentation, and finally the development of the various organs. About three-fourths of the book is concerned with organ development.

THE DISSECTION AND STUDY OF THE SHEEP'S BRAIN *as an Introduction to the Study of the Human Brain.*

By James Wilkie. Foreword by T. B.

Johnston. Oxford University Press, New York; Humphrey Milford, London. $2.25. $7\frac{1}{4} \times 4\frac{3}{4}$; xv + 95; 1937.

While this book is primarily concerned with the brain of the sheep, the author frequently refers to the human brain for the purposes of comparative anatomy. An excellent guide for pre-medical as well as medical and general students of brain anatomy. It is well illustrated and indexed.

PHYSIOLOGY AND PATHOLOGY

OCCIDENTAL THERAPEUTICS IN THE NETHERLANDS EAST INDIES DURING THREE CENTURIES OF NETHERLANDS SETTLEMENT. (1600–1900).

By D. Schoute. Netherlands Indies Public Health Service, Batavia. $10\frac{5}{8} \times 7\frac{1}{2}$; iv + 214; 1937.

In this book is given, in abbreviated form, the substance of two larger volumes (printed respectively in 1929 and 1935, in Dutch) on the history of medicine and surgery and the development of sanitary measures during three centuries in the East Indian Archipelago. It is a history of the struggles of the United Netherlands East India Company to keep illness and death rates reduced to a minimum during the long voyages around the Cape of Good Hope and of the growth of hospital and medical service in the Dutch East Indies.

We read that in 1621 "De Gouden Leeuw made an unheard-of short trip" (4 mos., 4 days) from the Netherlands to Batavia. Death rates aboard ship were frequently appallingly high. The Princesse Royale (sailing in 1647, 212 days at sea, 300 crew) lost 132 of the crew by death and "arrived, nearly all hands sick in berths." Scurvy and foul drinking water or no water at all were the chief worries of the early voyagers. When the drinking water became wormy it sometimes was put through a process of purification "as best as could be done by dropping glowing iron or a big heated cannon-ball into it, mostly a thirty-pounder." Sometimes on a voyage greatly prolonged by adverse winds, when scurvy was particularly bad, the ship would stop at St. Helena or the Cape. With an abundance of fresh vegetables and fruits recovery of passengers and crew seemed little short of miraculous, but more often than not, after a short time again at sea, a new crop of troubles appeared in the form of fevers which were much more to be dreaded than scurvy.

The volume is well documented and contains indexes of personal names and of subjects. Of particular interest to physicians and public health workers will be the sections dealing with the evolution of hospital service in the Dutch East Indies, and the steps which were taken to improve general health conditions. The author includes many interesting mortality tables which he has collected from various authentic sources.

THE FIGHT FOR LIFE.

By Paul de Kruif. Harcourt, Brace and Co., New York. $3.00. $8\frac{1}{2} \times 5\frac{3}{4}$; [10] + 342; 1938.

This book continues the appeal for wider and more intelligent application of existing medical knowledge and techniques to humanity in the mass that was begun in the author's preceding volume *Why Keep Them Alive?* It is written in the fine and gallant spirit of a passionate crusader. Righteous indignation bursts forth on nearly every page at society's too calm and placid acceptance of some of the more glaring imperfections of a world that indubitably falls short of complete impeccancy in a number of respects. The principal targets this time include maternal mortality, gonorrhea, syphilis, tuberculosis, and poliomyelitis.

Whether one prefers the warmly emotional or the coldly intellectual technique for diffusing ideas and information is a matter of taste, in respect of which individual human beings differ. But regardless of tastes there surely can be but few persons, if any, who will not find this book fascinatingly interesting. Its deep sincerity is evident, and its scientific reporting is meticulously accurate in detail.

THE PATIENT AND THE WEATHER. *Volume IV, Part 3. Organic Disease. Surgical Problems.*

By William F. Petersen with the assistance of Margaret E. Milliken. Edwards Bros., Ann Arbor, Mich. $10.00. $10\frac{3}{4} \times 8\frac{1}{4}$; xxxvii + 651; 1938.

In this volume the author deals with some acute abdominal and surgical diseases in connection with meteorological influences after having discussed in former volumes this influence on general physiology, pathology and some groups of special diseases, as, for instance, psychosis (cf. Q. R. B., Vol. 10, No. 3, 1935). The surgical diseases of the abdomen like ulcer of the stomach, gall bladder diseases, acute pancreatitis, appendicitis, etc. are considered from the point of view that they represent acute vascular and smooth muscle dysfunction, so that a uniform mechanism underlies them. The author presents for every sort of disease a good many detailed medical histories with meteorographs to illustrate the different clinical events. This casuistic material is of the greatest interest in many directions, but it should be completed by an adequate statistical technique to prove that there is really a coincidence or at least a correlation between the supposed abnormal fluctuations in vasoconstriction and dilatations and some components of the weather, like barometric pressure, humidity or sun spot emanations, etc. The weather, as the atmospheric environment, plays evidently an important rôle in the course of many diseases, but up to the present its influence has not been made as clear as that of such other environmental factors as parasites, diet and social conditions. Perhaps this exhaustive work will show the way for a more exact study.

THE SPAN OF LIFE.

By William M. Malisoff. J. B. Lippincott Co., Philadelphia. $2.50. $8 \times 5\frac{5}{8}$; 339; 1937.

This treatise professes a profound faith in science, and yet never loses an opportunity of misrepresenting the results of careful, thorough, and honest scientific work. The author starts by discussing length of life and its underlying phenomena, passes to theories of aging, and finally gets on to the prolongation of life. The style throughout is cheap and tawdry wisecracking journalese. Optimism about the future in spite of the present sad state of affairs is professed, and a foundation is proposed to synthesize all that the many branches of science have to offer. Possibilities are even suggested for its financial maintenance. But where are the ones to start Malisoff's new Coöperative Research Foundation for the attainment of astounding age going to come from when all of "the grey little men of science, proud of their techniques and miniature certainties—have moved into a cosy apartment of science and there they will stay, away from the chaos of the pioneering front?" Perhaps the idea is that unscientific pioneers will do the job.

Doubtless a poorer book than this *could* be written on longevity.

WHY GROW OLD? *A Guide-Book for the Man Who Seeks to Remain Physically and Mentally Young.*

By Frank S. Caprio and Owsley Grant. Maxwell Droke, Indianapolis. $2.50. $8\frac{1}{4} \times 5\frac{1}{2}$; [18] + 204; 1937.

The past several years have seen a multiplicity of books of the general type that conveys vast encouragement that "life begins at forty" in a real sense, and that if a man takes care of his heart, his stomach, his waistline, his gonads, and his prostate, he can enjoy life a long way past his allotted three score and ten years. The present volume falls exactly into the above mentioned general type, and reveals nothing original with regard to either material or presentation. As a matter of fact a man of the meanest intelligence will find little here that he does not already know, either as a result of trial and error, of reading magazine advertisements, or of exercising a little common sense. Each chapter of the book is summarized in the form of either a question list, or a do and don't list with regard to such topics as diet, exercise, sleep, sex, and mental traits. There is no index.

Symposium on Hormones. *Sigma Xi Lectures for 1936–37 Ohio State University.*

Various Authors. Ohio Journal of Science, Vol. 37. $1.00. $9\frac{3}{4} \times 6\frac{3}{4}$; 315–463; 1937 (paper).

The following papers are included in this symposium: *The Growth Hormones Found in Plants,* by George S. Avery, Jr.; *Responses of Plants to Hormone-Like Substances,* by P. W. Zimmerman; *Nervous System and Internal Secretion,* by Leon Asher; *The Gastrointestinal Hormones of Autacoids,* by A. C. Ivy; *The Prolongation of Insulin Action,* by C. H. Best; *The Male Sex Hormone—Some Factors Controlling Its Production and Some of Its Effects on the Reproductive Organs,* by Warren O. Nelson; *The Female Sex Hormones,* by Hans O. Haterius; *The Thyroid Hormone,* by David Marine; *Hormones of the Adrenal Gland,* by Frank A. Hartman; *The Hormones of the Anterior Pituitary,* by Oscar Riddle.

None of the authors has attempted to make an exhaustive survey of his subject. Instead each has taken some more or less particular aspect of the selected topic for elucidation and brief discussion. A reference list or bibliography is appended in all cases except in the paper by Asher.

Handbuch der biologischen Arbeitsmethoden. *Lieferung 467. Abt. V, Methoden zum Studium der Funktionen der einzelnen Organe des tierischen Organismus, Teil 10, Heft 7. Allgemeine und vergleichende Physiologie (Ergänzung zu Abt. V, Teil 2).* Containing following articles: *Lichtquellen für wissenschaftliche Zwecke,* by Marie Wreschner; *Instrumente und Methoden zur elektrischen Strom- und Spannungsmessung,* by Marie Wreschner; *Zell- und Gewebetrennung,* by Martin Behrens; *Fraktionierung und Reindarstellung organischer Substanzen nach dem Prinzip der Tswettschen Adsorptionsmethode,* by Alfred Winterstein.

Urban and Schwarzenberg, Berlin. RM. 13. 10 x 7; 239; 1938 (paper).

The first, a short paper, illustrates various sources of light adequate for scientific work. The second article, occupying about half the pages of this *Lieferung* treats methods of measuring electric current and voltage. In the third, Behrens describes a new method for separating cells and tissues which involves initial freezing of the organs. Winterstein treats the successful utilization of Tswett's chromatographic adsorption method for fractionating and purifying colorless organic substances. This method, first perfected by Tswett in 1906 for coloring matter of plants, was not universally known as the work was published in Russian, but received further impetus at its rediscovery by Kuhn in 1931.

Pavlov and His School. *The Theory of Conditioned Reflexes.*

By Y. P. Frolov. Oxford University Press, New York. $4.00. $8\frac{1}{2} \times 5\frac{1}{2}$; xix + 291 + 26 plates; 1937.

The method and reasoning by which Pavlov and his school attacked the biological problem of nervous activity through the aid of conditioned reflexes form the basis of this book. The context is made clear enough to be intelligible to the average biologist. Although there is little bibliographical material, the reader can obtain some insight of the character of the great Russian physiologist from the way he conducted his laboratory and by the great reverence he inspired in his co-workers. The translation has been excellently rendered by C. P. Dutt. There are several illustrations and an index.

Medical Records in the Hospital.

By Malcolm T. MacEachern. Physicians' Record Co., Chicago. $3.00. $9\frac{5}{8} \times 6\frac{1}{2}$; xv + 374 + 1 folding chart; 1937.

In this book the Associate Director of the American College of Surgeons briefly describes the development of the medical record and the important rôle it plays in clinical and research medicine; presents 99 sample forms for use in collecting the data comprising the record; gives suggestions as to the indexing, filing and storing of histories; and includes a great deal of general information which makes the book a useful manual for the record librarian.

An addendum of 44 pages contains a

list of journals, texts and monographs prepared by the Department of Library Research of the American College of Surgeons to aid in the selection of books for the hospital library. There is a bibliography of 34 titles and an index.

THE BIOLOGY OF PNEUMOCOCCUS. *The Bacteriological, Biochemical, and Immunological Characters and Activities of Diplococcus Pneumoniae.*

By Benjamin White with the collaboration of Elliott S. Robinson and Laverne A. Barnes. The Commonwealth Fund, New York; Oxford University Press, London. $4.50. $9\frac{3}{8} \times 6$; xvii + 799 + 10 plates; 1938.

This exhaustive monograph will become indispensable for everyone who is working on the experimental, clinical or epidemiological phases of pneumococcus infection. The classification of pneumococci according to the different types, the knowledge of which is so important for treatment, is given with full details. Especially is all the literature on the bacteriological, biochemical and immunological characters of pneumonococci examined and reviewed. There is a bibliography of 1593 titles, covering more than 100 pages.

LE SYSTÈME NERVEUX VÉGÉTATIF.

By J. Tinel. Masson et Cie, Paris. 160 francs. $10\frac{1}{8} \times 6\frac{3}{4}$; 847; 1937 (paper).

This comprehensive treatise includes a survey of the physiology, anatomy, and histology of the sympathetic and parasympathetic nervous systems; the action of these systems on the senses, muscular tonus, and trophic reflexes; anatomic and reflex syndromes involving the nervous system in certain diseases; hormonal and pharmaceutical influences; and therapeutic measures, surgical and medical, in neural diseases. A selected bibliography of only the most important or comprehensive works, an index and 603 excellent illustrations are included.

LES ACQUISITIONS NOUVELLES DE L'ENDOCRINOLOGIE. *Troisième Édition entièrement refondue et augmentée.*

By R. Rivoire. Masson et Cie, Paris. 45 francs. $9\frac{3}{4} \times 6\frac{1}{2}$; 264; 1937 (paper).

The appearance of a third edition of this work is testimony to its interest and success. The present edition has been entirely rewritten to include the newest findings, and two chapters, one on the thyroid and the other on the thymus and epiphysis have been added. For those not yet acquainted with this book it may be mentioned that it brings together the most important and definitive findings of physiological, biochemical, and clinical research on the endocrines. Unfortunately the work lacks a bibliography or documentation other than the mention of names.

THE HAIR AND SCALP. *A Clinical Study (With a Chapter on Hirsuties). Second Edition.*

By Agnes Savill. William Wood and Co., Baltimore. $4.75. $8\frac{1}{2} \times 5\frac{1}{2}$; viii + 309; 1937.

Following a summary of the structure and physiology of the hair, the principal diseases of the hair and scalp are arranged on a symptomatic basis. The probable etiology, clinical appearance, and alternative courses of treatment are described. The book is intended primarily for practicing physicians and medical students, but many cautions are directed at hairdressers. Lay people who are willing to wrestle with medical terminology can obtain sound advice on the relation of general bodily health to hair growth. The illustrations are adequate and there is an excellent index.

MACLEOD'S PHYSIOLOGY IN MODERN MEDICINE. *Eighth Edition.*

Edited by Philip Bard, with the collaboration of Henry C. Bazett, George R. Cowgill, Harry Eagle, Chalmers L. Gemmill, Magnus I. Gregersen, Roy G. Hoskins, J. M. D. Olmsted and Carl F. Schmidt. C. V.

Mosby Co., St. Louis. $8.50. $9\frac{1}{4} \times 6\frac{1}{2}$; xxxv + 1051; 1938.

Due to the many developments in the physiological sciences the distinguished group of physiologists who take part in the revision of this important text have practically rewritten the subject matter. Professor Bard states in the preface that "the present volume makes no pretense of being a text-book of 'applied' physiology." Clinical material, however, has been called upon when it illustrates particularly well fundamental physiological principles. The volume is a well-rounded, authoritative treatise—a worthy successor to the earlier editions. It is abundantly illustrated, extensively documented and has a useful index.

TEXTBOOK OF EXPERIMENTAL SURGERY.

By J. Markowitz. William Wood and Co., Baltimore. $7.00. 9 x 6; xv + 527; 1937.

Experimental surgery, according to Dr. Markowitz, plays too small a part in the training of surgical students. It is by this means that one obtains practice in the fundamental but very essential techniques for later use in the human operating room. The importance of a mastery of physiology by the surgeon is especially stressed. Accompanied by numerous clear illustrations, the text material describes in detail the operative methods for use on various tissues and organ systems of experimental animals, particularly dogs. A fine book for the medical student or clinician.

THE RABBIT TEST *for the Detection of Chorionic Tissue in the Body and the Determination of Its Proliferative Activity.*

By S. B. Anklesaria. With a Foreword by Emil Novak. Popular Book Depot, Bombay H. K. Lewis and Co., London. 12s. $9\frac{3}{4} \times 6\frac{3}{4}$; xvi + 161 + 8 plates + 2 graphs; 1937.

After giving a brief review of female sex hormology (human) and the technic of the original Aschheim Zondek test, the author discusses in considerable detail reproduction and ovulation in the rabbit, advantages of the rabbit test over other biological tests, technic, possible fallacies, clinical significance of the biological pregnancy tests, etc. The volume contains a number of illustrations, a lengthy bibliography and author and subject indexes. Emil Novak contributes a preface.

MAN, BREAD AND DESTINY. *The Story of Man's Food.*

By C. C. Furnas and S. M. Furnas. Williams and Wilkins Co., Baltimore. $3.00. $8\frac{1}{2} \times 5\frac{3}{4}$; xix + 364; 1937.

By a judicious mixture of facts, anecdotes, and theories, the authors have succeeded in presenting a book which is an informative and far from dull account of the chemical constituents of food, metabolism, nutrition, diet, simple cooking rules, and food fads. It is well written and superior to the many popular and semi-popular publications on the subject. There is an extensive and adequate bibliography.

RELATION OF STABLE ENVIRONMENT TO MILK PRODUCTION. *U. S. Department of Agriculture. Technical Bulletin No. 591.*

By M. A. R. Kelley and I. W. Rupel. Government Printing Office, Washington. 15 cents. $9\frac{1}{4} \times 6$; 60; 1937 (paper).

This pamphlet reports the results of a series of tests on selected cows as to the relationship of milk production to environmental conditions such as stables, food, weather and temperature. The effects of stable temperature and temperature changes on milk yield and on physical factors such as respiration are discussed. The problem of water consumption and stable conditions are also considered.

L'ACÉTYLCHOLINE ET L'ADRÉNALINE. *Leur Rôle dans la Transmission de l'Influx Nerveux. Bibliothèque Scientifique Belge, Section Biologique.*

By Z. M. Bacq. Masson et Cie, Paris.

20 francs. $7\frac{1}{2} \times 5$; 114 + 1 folding chart; 1937 (paper).

This book discusses concisely the problems in the chemical transmission of nervous impulses, and the progress so far made in their solution. It is also a valuable contribution to the history of the subject. Dr. Bacq discusses sympathetic, parasympathetic, and motor nerve transmission; the theory and formation of chemical mediators; and comparative nervous physiology.

THE ENDOCRINES IN THEORY AND PRACTICE. *Articles Republished from the British Medical Journal.*

By Various Authors. P. Blakiston's Son and Co., Philadelphia. $3.50. $8 \times 5\frac{3}{8}$; ix + 278; 1937.

These 28 articles, by outstanding authorities, summing up the solid achievements in the field of endocrinology, will be useful to the student, the general practitioner, and the specialist. They furnish a much needed orientation in a fertile and rapidly developing subject which was in danger of running out of balance. Brief bibliographies are given at the end of each section and there is an adequate index.

COLD SPRING HARBOR SYMPOSIA ON QUANTITATIVE BIOLOGY. *Volume V.*

Edited by Eric Ponder. The Biological Laboratory, Cold Spring Harbor, L.I. $4.50. $10\frac{3}{4} \times 7\frac{3}{4}$; xv + 433; 1937.

The symposia held each summer at Cold Spring Harbor are on some selected aspect of quantitative biology. The subject for the summer of '37 was internal secretions and in this volume are published the 43 papers presented, together with the discussions. Many of the papers are accompanied by figures or tables and necessarily brief reference lists. There is a general index.

THE TRANSMISSION OF DISEASE BY FLIES. *U. S. Treasury Department, Public Health Service. Supplement No. 29 to the Public Health Reports.*

By Cornelius B. Philip. Government Printing Office, Washington. 10 cents. $9\frac{1}{4} \times 6$; ii + 22 + 3 plates; 1937 (paper).

To bring to the public mind the fact that flies play an important rôle in the transmission of disease is the purpose of this government publication. The house fly is used as an illustration of the order. Sanitary measures for combating these insects and some of the diseases that they carry are discussed.

DIE GESCHLECHTSKRANKHEITEN UND IHRE GEFAHREN FÜR DAS VOLK. *Zweite Auflage.*

By Johannes Breger. R. v. Decker's Verlag, G. Schenck, Berlin. RM. 4. (paper); RM. 5. (cloth). $9\frac{3}{4} \times 7$; 150; 1937.

A useful description of the damages caused by the venereal diseases. This popular presentation is made for the enlightenment of the people in Germany.

BIOCHEMISTRY

A SHORT HISTORY OF CHEMISTRY.

By J. R. Partington. The Macmillan Co., New York. $2.50. $7\frac{3}{4} \times 5\frac{1}{4}$; xiii + 386; 1937.

A noted scholar in the history of chemistry presents here a valuable account of the progress of his science from earliest times through theories of radioactivity and atomic structure to approximately 1935. The material in this little book is well presented but stated so concisely that its appeal will be limited to students and teachers of elementary courses in the history of chemistry rather than to either laymen or specialists. Relative to its size, it is astonishingly replete with information, although of necessity it treats but lightly of the chemical fields lying within the penumbra of the science, such as physical, biological, industrial chemistry, and neglects details throughout except for the briefest of references. When a contribution is merely mentioned by a phrase in passing or in a listing at the end of a chapter, it would be highly desirable for the author to state a reference to which the reader can turn if his interest impels him in that direction. Only through such assistance can still another brief account of the history of chemistry justify its publication in a field in which

there are already a fairly large number of short works.

Although Partington's book includes more references than do most of its competitors, and can be favorably compared in several respects to such rivals as Hilditch's *Concise History of Chemistry* or Moore's *History of Chemistry*, it nevertheless will probably be considered simply another elementary text. What is needed today is not a repetition of what is already on the market, but rather a comprehensive treatise, similar to von Meyer's great standard work which is no longer procurable, detailing the progress of chemistry in its various branches to the present day.

KATALYSE UND DETERMINISMUS. *Ein Beitrag zur Philosophie der Chemie.*

By Alwin Mittasch. Julius Springer, Berlin. RM. 9.60. $8\frac{1}{2} \times 5\frac{1}{2}$; ix + 203; 1938 (paper).

The author deplores the fact that for some time chemistry has not been favored with the same attention as physics or biology among philosophers. Here he attempts to remedy this matter, and his specialty, catalysis, is especially suited to the purpose. "Above all," he writes, "one should no longer speak of *Natural Causation* (or determinism) without taking consideration of the definite rôle which catalysis plays in it," and further, that this determinism is no one-sided mechanistic one but that "a broader frame is necessary, in which freedom as well as force can exist." Among the subjects treated are: catalytic causality as a form of stimulus causality, chemical reactions of catalysts, the relation of catalytic causality to other types of causality, the place of catalysts in the order of causalities, and what is included in the concept of determinism. Obviously it is impossible to adequately discuss these points in a short review. The book is well documented, contains a bibliography and is well worth reading by the philosophically-minded scientist.

PRÉCIS DE CHIMIE ORGANIQUE.

By Victor Grignard. Published under the direction of Roger Grignard and Jean Colonge. Preface by G. Urbain. Masson et Cie, Paris. 150 francs (paper); 175 francs (bound). $9\frac{3}{4} \times 6\frac{1}{2}$; xiv + 774; 1937.

This excellent textbook, essentially the work of the senior author, who is considered one of the greatest present-day French chemists, has been posthumously published and carefully edited by his son and his collaborator. Although the whole subject of organic chemistry is here treated as a practical rather than an abstract science, the theories of physical chemistry are presented at some length. The methods of degradation are systematized as far as possible and presented for the first time in a French text intended for elementary classes. This work is indexed for subjects and authors. The *Précis* may also serve as an introduction to the more comprehensive fifteen volume *Traité de Chimie Organique*, five volumes of which have already been published.

FOOD VALUES OF PORTIONS COMMONLY SERVED.

Compiled by Anna DeP. Bowes and Charles F. Church. (Obtainable from the Authors, Philadelphia Child Health Society, 311 S. Juniper St., Philadelphia). 50 cents. 10 x 7; [6] + 12; 1937 (paper).

The purpose of this work is, in the words of its authors, "to supply authoritative data on the nutritional values of foods in a form for quick and easy reference." The foods are arranged alphabetically as are also the classes under which the foods are grouped. The system of using portions frequently served as a basis of calculation lends itself very practically to a study of comparative food values and an approximate analysis of diets. Beer lovers who are viewing their disappearing waist lines with well-founded apprehension, happily will be able to find herein proof that a certain much touted dairy health beverage contains 166 calories in comparison with the mere 101 calories of an equivalent amount of lager beer.

FUNDAMENTALS OF BIOCHEMISTRY WITH LABORATORY EXPERIMENTS.

By Carl L. A. Schmidt and Frank W. Allen.

McGraw-Hill Book Co., New York. $3.00. 8 x 5½; xiv + 388; 1938.

This text has for its object "to present sufficient discussion of the subject to give the student a background for his laboratory work. The aim of the laboratory work is to illustrate some of the facts of biochemistry." The book is divided into three parts: (1) a discussion of the facts of biochemistry, (2) laboratory experiments which all the students may carry out during the usual course in biochemistry, and (3) special experiments for groups of two or more students. There is much tabular matter throughout the text, lists of suggested reading and an adequate index.

Reports of the Biochemical Research Foundation of the Franklin Institute. *Vol. IV, 1936–1937.*

Franklin Institute, Philadelphia. 9¾ x 7; collection of reprints.

A collection of papers on bio-chemistry, histo-chemistry, metabolism of normal and tumor tissues, and five papers on cancer statistics. There is a somewhat pugnacious foreword by the Director.

SEX

A Study of the Greek Love-Names. *Including a Discussion of Paederasty and a Prosopographia. The Johns Hopkins University Studies in Archaeology, No. 23.*

By David M. Robinson and Edward J. Fluck. The Johns Hopkins Press, Baltimore. $3.00. 9⅛ x 6; vi + 204; 1937.

The aspect of Greek mores that later ages have found hardest to understand is their attitude towards paederasty. It is true that with a greater understanding of the causal mechanism of homosexuality we are less inclined than our forefathers to condemn it as criminal, but most of us would still regard it as pathological, an unfortunate maladjustment in the developing chain of sex reflexes. Few would echo Plato's eulogy of paederasty. Nevertheless, as the author of this book points out,

> though it be a faint unintelligibility to see just how essentially related, as cause and effect, were Greek moral-ideals and Greek art, still no progress in interpretive appreciation can ever be made if the layman behaves like an ostrich in the face of something distasteful and ignores its reality. To see only the dazzling shimmer of its reflection in Hellenic art without admitting the strange shadows that, to our sense, so enshroud the sensitive soul of the Greek, is as purblind an occupation as that of separating the known homosexuality of this people from the fine jewel of art it produced in the Athenian Akropolis.

One reflection of the place of paederasty in the Greek mores is the frequent occurrence of the formula: "The boy is beautiful" or "So-and-so is beautiful" on Greek vases. This book catalogues the various names that occur in this formula whether in literature or on vases and suggests identifications of the names with historical characters known from other sources.

About the time of the Peloponnesian War paederasty sank into disrepute, as may be seen by the change in attitude from Plato to Aristophanes. One result of this and of the linked rise in repute of heterosexual love is the more frequent occurrence of nude women in Greek sculpture, whereas before they had been draped and only the men nude. Another is the disappearance of love-names from Greek vases.

Sex, Custom and Psychopathology. *A Study of South African Pagan Natives.*

By B. J. F. Laubscher. Robert M. McBride and Co., New York. $5.00. 9¾ x 6¼; xv + 347 + 16 plates; 1938.

This interesting and well written volume —a study of the South Eastern Cape Bantu —is an endeavor to show the influence of the native's beliefs and customs on psychopathology in a primitive culture. In detail Dr. Laubscher, who is on the staff of the Queenstown Mental Hospital, discusses the folklore and mythological characters which play an uncannily real part in the life of the native. His chapter on diviners and sorcerers and the remarkable feats of these men is also one of great interest. However, he fails to explain their superhuman powers attributing them to "psychic sensitivity," "because the word 'intuition' does not cover all its phases." Further chapters describe native

sacrifices, burial and marriage rites, the complicated rituals that involve circumcision and the initiation of the adolescent into manhood. The author's psychiatric interpretation of the various and complex native rites and their bearing on psychoses verge on the Freudian, but his great abundance of factual material, accurately and painstakingly collected, should be of value to the anthropologist as well as entertaining to the layman.

In conclusion the author writes of the native's own conception of mental disorder and its confusion with witchcraft, and devotes two chapters to his native material in the Mental Hospital. He finds that the behavior of the native mental patients is similar to that of European psychotics and suggests that too much emphasis has been laid on the environment in schizophrenia and not enough upon constitutional factors. The final chapter discusses the prevalence of stock theft and homicide and the psychological make-up and culture conducive to these crimes. There is also a chapter on sexual offences.

An appendix supplements the chapter on hospital material containing family histories of some of the native psychotics showing that often members of their families were either witch-doctors or were considered bewitched by their tribesmen. Excellent and interesting photographs accompany the text and there is an adequate index.

Die Störungen der Sexualfunktion bei Mann und Weib.

By Ludwig Chiavacci. Introduction by Otto Pötzl. Franz Deuticke, Leipzig and Wien. 4.60 marks (paper); 5.40 marks (bound). $9\frac{1}{2} \times 6\frac{3}{8}$; vi + 145; 1938.

This brochure is a mixture of common sense and dubious procedures. The title, which implies a discussion of the disorders of sexual function, is misleading. The book deals mainly with impotence, fifteen pages only are devoted to sterility, and many definite disorders such as nymphomania, satyrism, vaginism, etc. are not touched upon at all. The author sensibly states that frigidity and impotence are not diseases but symptom complexes and for this reason treatment should be varied according to the individual. However, his recommendation that infidelity should be suggested very subtly by the physician in those cases where the impotence of the husband might be due to the frigidity or sexual inadequacy of the wife seems dubious, let alone under-handed. His discussion of treatment for impotence otherwise is an enumeration of patented endocrine preparations, especially the pluriglandular kinds that have so far not found great favor among American endocrinologists. The treatment of sterility is likewise dismissed with a list of various procedures and is of little practical use to the specialist. The book might however serve as a cram survey of the subject to the medical student.

Step by Step in Sex Education.

By Edith H. Swift. The Macmillan Co., New York. \$2.00. $8 \times 5\frac{1}{2}$; xiii + 207; 1938.

In dialogue form this book aims to impart the ideal way of instructing the young in the facts of life. It is not a repetition of the old bee and flower sex education, for Dr. Swift is of the firm opinion that parents should inform their children about "the seamy side of life" before they have a chance to learn it from other and less poetic sources. Her theory may be a sound one, but the manner in which it is portrayed is enough to nauseate the least squeamish. Perhaps the blame may be laid on the bad English and complete artificiality of the author's model characters. At any rate, the whole book leaves one with the impression that these parents spend their entire lives lying in wait to corner their unsuspecting youngsters in order to stuff them to the gills ("casual-like") with sex information, and that before the children are even house-broken they are none too subtly being modelled into ideal mates and parents.

This volume may be of use to some bewildered parents, but it is extremely doubtful that any real children would be

as cooperative as the idealistic son and daughter of this fairy tale.

Love and Happiness. *Intimate Problems of the Modern Woman.*

By I. M. Hotep. With a Prefactory note by Logan Clendening. Alfred A. Knopf, New York. \$2.00. $7\frac{1}{2} \times 5\frac{1}{4}$; ix + 235 + vii; 1938.

Here is a guidebook for the woman who would seek sexual solace in liaison. The author states that he is not writing for the uneducated girl, because she will never read his words. Yet the person with a moderate degree of intelligence could about as well solve her problem with *Love and Happiness* in a dusky corner of a bookshelf. The folly of preaching morality is here recognized, but not the equal folly of preaching against the repressive hand of the church, social taboos, and political legislation. The material is divided into problems of the girl from 15 to 25, the woman from 26 to 40, the married woman, and the widow.

Mentality and Homosexuality.

By Samuel Kahn. Meador Publishing Co., Boston. \$3.00. $8 \times 5\frac{1}{2}$; 249; 1937.

The homosexuals represented in this study were inmates in the New York Penitentiary for men, and the Women's Workhouse and Correction Hospital for Women. The histories, including the sexual, were obtained by means of personal interviews and supplementary data were provided by the physical examinations and the mental testing. In his general conclusions Dr. Kahn indicates the factors that he considers the most important as concerns the diagnosis, treatment, and prevention of homosexuality. There is no index.

BIOMETRY

Random Variables and Probability Distributions.

By Harald Cramér. The University Press, Cambridge; The Macmillan Co., New York. \$2.25. $8\frac{5}{8} \times 5\frac{1}{2}$; [8] + 121; 1937 (paper).

There have been various approaches to the theory of probability. The first of these in point of time supposed that the ways in which an event can happen are divisible into a number of equally probable cases, such as heads and tails in a perfectly balanced coin, and defined the probability as the ratio of the number of favorable cases to the total number of cases. When we, however, broaden the application of the theory of probability to include more than games of chance it is not apparent what are the equally possible cases on which we are to base our probability. The second approach, accordingly, defines the probability of an event as the limit of ν/n as n approaches infinity, where n is the total number of observations and ν is the number of favorable observations. This is the approach followed by von Mises, and in this country by Coolidge. This definition, however, suffers from the defect that the ratio ν/n can be said to have a limit only in a highly Pickwickian sense. Actually as n increases the ratio in general shows a trend towards a limiting value but may exhibit fluctuations that any self-respecting mathematical function would consider beneath its dignity.

Cramér therefore chooses a purely postulational approach. If R is a set of points the probability P (s) of the sub-set S is defined as a non-negative number such that $P(R) = 1$ and $P(S_1 + S_2 + \ldots) = P(S_1) + P(S_2) + \ldots$ whose $S_1, S_2 \ldots$ have no common points. On this basis he develops the mathematical side of the theory, without any reference to the applications.

Statistical Methods *Applied to Experiments in Agriculture and Biology.*

By George W. Snedecor. Collegiate Press, Ames, Iowa. \$3.75. 9×6; xiii + 341; 1937.

In this very interesting textbook Professor Snedecor devotes more attention to the application of statistical methods to the interpretation of experiments than do most authors of statistical textbooks. The arrangement of the material is somewhat novel. The student is first intro-

duced to the chi-square method, then to the *t*-test, linear regression, correlation, large sample theory, analysis of variance and covariance, multiple regression and covariance, and curvilinear regression. The numerous examples not only give the student practice in applying the methods, but often illustrate significant points. Thus the examples on chi-square bring out the fact that the same percentage deviations are more significant the larger the populations on which they are based, while the same absolute deviations are less significant. Altogether this is an admirable text with special emphasis on the more recently developed statistical methods.

How to Use Pictorial Statistics.
By Rudolf Modley. Harper and Bros., New York. $3.00. $9\frac{1}{4}$ x $6\frac{1}{8}$; xviii + 170; 1937.

The advantages of graphic presentation of statistical data have long been recognized. However, a bar diagram or other graph, while it shows relations more clearly than a numerical table, is still highly abstract. Somewhat more than a decade ago Dr. Otto Neurath of Vienna began the development of a new form of graph which makes a more vivid and concrete appeal to the layman. This is essentially the division of a bar diagram into units, each of which is represented by a symbol of the subject. Thus a bar representing 15,000,000 people is replaced by fifteen schematized men, each representing a million people. The author, who has had a large share in introducing the method into this country, describes the technique of planning and making such pictorial graphs as well as pictorial diagrams not involving numerical relations. He sees a wide future for the method in schools and museums, and wherever quantitative data are to be presented to the layman. For a trained audience, however, he prefers the older forms of graphs.

Elements of Statistical Method.
By Albert E. Waugh. McGraw-Hill Book Co., New York. $3.50. 9 x 6; xv + 381; 1938.

This textbook differs little from the majority of statistical volumes that have recently appeared. It makes no attempt at specialization in a particular field, but gives a variety of examples, mostly from economics and biology. Development and derivation are largely omitted. Each chapter ending contains a number of exercises, designed primarily to evoke thought on the fundamental principles rather than on arithmetic methods. A series of appendices is included, giving short tables and a list of statistical books.

PSYCHOLOGY AND BEHAVIOR

The Mind of the Juror *as Judge of the Facts or the Layman's View of the Law.*
By Albert S. Osborn. Introduction by John H. Wigmore. Boyd Printing Company, Albany. (Also obtainable from the author 233 Broadway, New York). Library Edition $4.50; Students Edition $3.50. $9\frac{3}{8}$ x $6\frac{1}{8}$; xv + 239; 1937.

For nearly forty years Mr. Osborn has been an expert witness on documents before courts in nearly every state in the Union. It is natural that in this long service a shrewd observer should have accumulated a valuable fund of first hand knowledge on how our courts are actually run. In this book he presents his observations together with suggestions for the more efficient administration of justice. These include greater care in the selection of an intelligent type of jurors, abolition of the requirement of a unanimous verdict, appointment of judges for long terms instead of their election for short terms, repeal of the statutes which in many states gag the judge and leave the jury at the mercy of the sophistries of the contending lawyers, and exclusion from the bar, so far as possible, of those attorneys whose only interest in their profession is the amount of money they can acquire.

When a man of Mr. Osborn's essentially conservative temper and with his wide experience of our courts presents such an unflattering picture of their actual operation we may well pause and think. What

stands in the way of such reforms as he proposes?

The answer is easy and short and offensive; the legal profession in this country stands in the way. Next week steps toward this reform could begin, next month reform could be extended, and next year and the years following it could be completed if a majority of the legal profession earnestly desired reform.

The opponents of this proposed means of making the administration of the law more efficient by increasing the powers of the judge are the present day successors of those who originally took this power away. They are the advocates who strenuously object to any interference with their law practice by a man sitting on a bench as a judge. They say this frankly. One of these opponents in a burst of confidence said, 'It is difficult enough to defeat my opponent without also being obliged to defeat the judge.'

Every person interested in the more efficient administration of justice, whether lawyer or layman, should read and ponder this book.

A HERD OF RED DEER. *A Study in Animal Behaviour.*
By F. Fraser Darling. Oxford University Press, New York; Humphrey Milford, London. $5.50. $8\frac{3}{4} \times 5\frac{1}{2}$; x + 215 + 8 plates + 2 folding maps; 1937.

Most of life history studies have been among insects, birds, and a few other representatives of our animal life but the mammals have been sadly neglected, mainly because it requires a lot of time to observe their entire life history and because their size makes it infeasible to place them in cages for experimental purposes. By overcoming these difficulties, Darling has done a notable piece of work.

This book tries to give the plain tale of an animal's life, of the things it does and is trying to do, of its relations with its fellows and with men, and of the things to which, as long observation leads me to believe, it responds. The study can be considered in no way complete, for now, after two years, I am left with more problems to solve than I knew of at the outset.

The author is too modest when he says, "this book tries. . . ." For two years he stalked the deer of northwestern Scotland in all kinds of weather, and the result is this entertaining, penetrating, and scientifically important story of their doings. He found that the deer spend their lives in limited areas, that the hinds and stags have their own territories according to the seasons, that the stag's antlers are used as an erotic zone. There is also a wealth of material on movement, insect parasites, territory and population, reproduction, etc.

The observations are very honest and the author studiously avoids placing any anthropomorphic interpretations on behavior although he admits that the objections to anthropomorphism can be carried too far. Mammalogists, students of animal behavior, naturalists, deer raisers, and even deer hunters, will find this book absorbing and valuable. A notable contribution.

THE MEASUREMENT OF OUTCOMES OF PHYSICAL EDUCATION FOR COLLEGE WOMEN.
By Elizabeth Graybeal. University of Minnesota Press, Minneapolis. $1.00. $9\frac{1}{4} \times 6\frac{1}{4}$; viii + 80; 1937 (paper).

A controlled experiment was set up in which half of an entering class at the University of Minnesota participated in the regular required courses in physical education and the other half did not. The problem was to ascertain the differences, after a period of examination, in the outcome of four objective tests; attitudes, knowledge, motor ability, and posture. This pamphlet gives a detailed description of these tests and their outcome and many tables present the various scores of the two groups. It was found that those who participated in the prescribed courses demonstrated an improvement in the functions tested. While this was not always great, it was demonstrated so consistently that the author concludes that a college requirement in physical education would seem justifiable. A bibliography and appendix supplement the text.

L'INTELLIGENCE DES ANIMAUX. *Insectes sociaux, Chevaux mathématiciens et causeurs.*
By Maurice Lecat. Librairie Castaigne,

Brussels. 3 francs. $9\frac{3}{4} \times 6\frac{3}{8}$; 22; 1937 (paper).

This paper represents a chapter from the larger work of M. Lecat entitled *Le Maeterlinckianisme*, and deals with the philosopher's ideas on the intelligence of animals. The text is well documented with quotations from Maeterlinck's work, and is intended to give us some idea of the man and his work. The author very carefully refrains from making comments that would in any way influence the reader's opinions either of the man or of the value of his contributions. The strain upon the imagination in trying to visualize an ant that . . . "lives only for his God . . .", or a horse that can calculate the fourth root of 7,890,491 is so great that we are inclined to agree with other critics in saying that Maeterlinck was a mystic, and that some of his work would hardly withstand the test of scientific criticism.

THE INTERRELATIONSHIP OF DRIVES IN THE MALE ALBINO RAT. *II. Intercorrelations Between 47 Measures of Drives and of Learning. Comparative Psychology Monographs, Volume 14, Number 6, Serial Number 72.*

By E. E. Anderson. Johns Hopkins Press, Baltimore. \$1.50. $10 \times 6\frac{3}{4}$; 119; 1938 (paper).

Because in the earlier study the different drives were measured by a comparatively limited number of tests, this study in which 47 measures were obtained was undertaken to get more comprehensive data concerning the interrelationship of various measures to learning performance. The monograph gives a detailed description of the various tests and many tables illustrate the scores. In general the author found that the correlations of the earlier and less extensive study were higher than those of the present one, but there were many cases of essential agreement between the two. He believes that the inadequate preliminary adaptation and the lower degree of motivation for the test in the earlier experiment account for the discrepancies between the two studies.

A BIOLOGICAL APPROACH TO THE PROBLEM OF ABNORMAL BEHAVIOR.

By Milton Harrington. Science Press Printing Co., Lancaster, Pa. \$4.00. 9×6, 459; 1938.

Credit must be given to Harrington for following an earlier destructive criticism of the Freudian theory by this attempt at constructing a different angle of approach to the problem. The theory advocated here, that all abnormal behavior results from anatomical and physiological inadequacy of the individual, is obviously not an entirely new one. Perhaps somewhat insufficient acknowledgment is extended to others with a similar point of view. The author presents his arguments in a logical manner, but does not hesitate to recognize weaknesses in his purely mechanistic theory. The work deals with "psycho-physiology" or the mechanism of behavior, and "psycho-pathology" or the abnormal behavior which arises from this mechanism. Treatment of abnormality is left to a further volume.

THE PSYCHOLOGY OF EARLY GROWTH *Including Norms of Infant Behavior and a Method of Genetic Analysis.*

By Arnold Gesell and Helen Thompson, Assisted by Catherine S. Amatruda. The Macmillan Co., New York. \$4.00. $10 \times 7\frac{1}{2}$; ix + 290 + 15 plates; 1938.

This volume presents to a certain degree a continuation and elaboration of a former publication *The Mental Growth of the Preschool Child* that is now out of print. It deals especially with norms of infant behavior as to mental development in early childhood but includes also growth and anthropometric examinations. It is subdivided into three parts: I. Methods and procedures; II. Norms of infant growth; III. Analytical appraisal of infant growth status. As in the former investigations of the authors, it gives a great many different results of high value with regard to the mental development in infancy. These results are important for the study of child hygiene and individual psychology. For an anthropometric average the number of 107 infants studied and distributed among

different age groups may not be large enough.

Differential Psychology. *Individual and Group Differences in Behavior.*

By Anne Anastasi. The Macmillan Co., New York. \$2.75. $7\frac{7}{8}$ x $5\frac{1}{4}$; xvii + 615; 1937.

In this textbook, intended especially for the college student, the author has brought together and critically examined the facts and theories relating to variation in psychologic manifestations. In the first part Anastasi introduces the subject with an elementary exposition of the concept and measurement of variability and then proceeds to a discussion of the relation of psychologic differences to heredity, environment and physical constitution. In the second part the subject matter concerns subnormal intellect, genius, and the association of sex, race, and urbanism with certain mental characteristics. The soundness of the author's views and the adequate bibliography serve to make this an excellent work for its purpose.

Embryonic Motility and Sensitivity. *Monographs of the Society for Research in Child Development. Volume II, No. 6 (Serial No. 13).*

By W. Preyer. Translated from the Original German of "Specielle Physiologie des Embryo" by G. E. Coghill and Wolfram K. Legner. Society for Research in Child Development, National Research Council, Washington, D. C. \$1.00. 9 x 6; v + 115; 1937 (paper).

An important translation. The original (of which this is only a part, beginning with Chapt. 6) appeared in 1885 and "summarizes the earlier literature on the physiology of the embryo with Preyer's critical consideration and includes Preyer's own unsurpassed observations on embryos." With the present development of interest in genetic psychology and the growing awareness that behavior begins before birth, Preyer's monograph returns again to a well-deserved place on biological book-shelves. The translation is so arranged as to facilitate comparison with the original. The bibliography of over 500 titles is included and there is an index of authors.

Personality *in Formation and Action.*

By William Healy. W. W. Norton and Co., New York. \$2.00. 8 x $5\frac{1}{2}$; 204; 1938.

The author is the director of the Judge Baker Guidance Center, Boston, and this account is based upon 30 years experience with a great variety of personality problems. Psychological theories do not rate a great deal of space, and make way for the common sense of experience and the insight that is presumed to follow upon the heels of the latter. While we agree that it is the common sense that gives to the book what value it has, we are not so sure that the insight is of the sort that goes very far beneath the surface.

The Backward Child.

By Cyril Burt. D. Appleton-Century Co., New York. \$5.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; xx + 694; 1937.

A comprehensive discussion of the interrelations of the factors determining backwardness, physical and mental defects, poor environment, etc. The analysis of the sensory and motor defects is based upon a large body of experimental evidence, particularly so in the cases of speech defects and left-handedness. Considerations of general intelligence, temperament, character, and environment figure prominently in the discussions on the diagnosis and treatment of backwardness. There is an appendix devoted to the curves of growth, and another to the statistical criteria.

The Quest of the Overself.

By Paul Brunton. E. P. Dutton and Co., New York. \$3.00. $8\frac{1}{2}$ x $5\frac{7}{8}$; 304; 1938.

According to the author this book is an explanation of a system of Yoga altered and adapted to suit modern Western needs.

To the reviewer a system of breathing exercises as a means of attaining spiritual truth has always seemed slightly grotesque. In a quotation from Carrel there is an amusing error: "It is known that certain animals such as the small anthropoid, tardigradum, stop their metabolism when they are dried." Surely for "anthropoid" we should read "arthropod."

THE MANAGEMENT OF EARLY INFANCY. PUBERTY AND ADOLESCENCE. THE PSYCHOLOGICAL APPROACH. THE NEUROTIC CHARACTER. *Individual Psychology Medical Pamphlets—No. 18.*

By E. Joyce Partridge, H. Crichton-Miller, T. A. Ross and F. G. Crookshank. C. W. Daniel Co., London. 2s. 6d. net. $8\frac{1}{2}$ x $5\frac{1}{2}$; 64; 1937 (paper).

The four papers presented in this pamphlet, by psychologists of standing, formed part of a symposium on Mental Health in Childhood and Adolescence. They were originally delivered before the Medical Society of Individual Psychology, London.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

DE VERITATE.

By Edward, Lord Herbert of Cherbury. Translated with an Introduction by Meyrick H. Carré. J. W. Arrowsmith, Bristol, England. 12s. 6d. net. $8\frac{1}{2}$ x $5\frac{1}{2}$; 334; 1937.

This is a translation from Herbert's rather crabbed Latin of the first purely metaphysical work written by an Englishman. Partly because of the difficulties of his style and partly because the main current of English metaphysics has been empiricist, Herbert has been little more than a name to most English students of philosophy. As Mr. Carré points out in his introduction, Herbert in his emphasis on Natural Instinct and its product, the Common Nations, is a precursor of Kant and his *a priori* forms of knowledge. Indeed except for Reid and his followers, Herbert's affinities are with the Continental rather than the British metaphysicians.

In placing at the centre of his scheme of knowledge self-evident judgments of reason, Herbert stands sharply contrasted from the empirical method powerfully advocated by his great contemporary. But he penetrates far more deeply into the metaphysics of knowledge than Bacon, examining assumptions which the latter takes for granted. For the first time in English thought—and it is not easy to find a parallel elsewhere in Europe—a systematic and comprehensive enquiry into the complex processes of knowing is undertaken. That the analysis is often confused and superficial does not detract from the merit of the insight which is achieved. Even in the psychological portions of the work there is, as we have noted, remarkable foreshadowing of later theories. But in its central conception of the organic relation between the impulse of Nature and human thought, in its theory of the reciprocity between objects and the understanding, and in its discussions of the *a priori* foundations of knowledge, it expresses philosophical ideas which look beyond the whole course of English empiricism, by which for nearly two centuries they were submerged.

MAKING PICTURES WITH THE MINIATURE CAMERA. *A Working Manual.*

By Jacob Deschin. Whittlesey House, McGraw-Hill Book Co., New York. \$3.00. 9 x 6; xii + 156 + 47 plates; 1937.

For the amateur or even the professional miniature photographer who is seeking exact and detailed directions in every phase of his art this little volume is of extreme value. Mr. Deschin writes as one with experience not only in the art of photography, but also in the field of editing. His editorial work has been seen in several journals in the form of keen and understandable articles on every phase of his work. The book is divided into four parts, namely: (1) Taking the Picture, which includes a discussion of techniques in "shooting" every conceivable type of picture; (2) Making the Negative, which contains all the salient points on equipment and technique in developing; (3) Making the Picture, which gives all the details of printing, enlarging, and touching up the picture; and (4) A Miniature Camera Gallery of 47 plates characteristic of the type of photography displayed in the countless picture magazines found on all our corner news stands. The first three sections are well illustrated by diagrams and line drawings. The volume is completed with a glossary of photographic terms and an index.

MINIATURE PHOTOGRAPHY *From One Amateur to Another.*

By Richard L. Simon. Introduction by Laurence Stallings. Simon and Schuster, New York. $1.75. $7\frac{3}{4}$ x $5\frac{1}{2}$; x + 168 + 30 plates; 1937.

This little guide to miniature photography has been written to supply the solutions to some of the many problems and questions that confront the candid cameraman as he pursues his hobby along ever-widening trails. Though we could hardly say that the work was the product of a literary genius, it is nevertheless clearly and interestingly written, and spiced up a bit by an occasional photograph illustrating good or bad technique in some phase of the making of the picture. The content is made up of a discussion of such topics as type of equipment, expenses, exposing, developing and printing of pictures. Included also are some notes on publications and gadgets, appendices A and B on how to work several types of cameras, and an index.

SCIENCE IN OUR LIVES.

By Benjamin C. Gruenberg and Samuel P. Unzicker. World Book Co., Yonkers-on-Hudson, N. Y. $1.76. $7\frac{1}{2}$ x $5\frac{3}{8}$; xiv + 750; 1938.

A praiseworthy comprehensive study of general science. Simple vocabulary, uncomplicated sentences, and concrete, familiar examples make this a practical, teachable textbook. Herein all science is presented as a related picture, and the social significance of scientific achievement is stressed. The contents are divided into two parts each of which is subdivided into four units. Air, water, fire, and earth are the four "elements" developed in Part I. Work, electricity, life in respect to nutrition, environment, reproduction, heredity, sound and hearing, light and sight, and reflexes and instincts are dealt with in Part II as part of man's control over nature. There are also 97 problems to stimulate the natural curiosity of the student and over 300 described activities for practical demonstrations of scientific procedure.

BOOKS AND DOCUMENTS. *Dating, Permanence and Preservation.*

By Julius Grant. Chemical Publishing Co., New York. $4.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; xii + 218 + 12 plates; 1937.

Part I deals with the origin and development of paper from the point of view of dating characteristics and describes tests which indicate the presence or absence of these characteristics. There is a special chapter on the use of fluorescence in ultraviolet light. Part II deals exhaustively with the durability, permanence and preservation of books and documents. The volume will interest librarians, antiquarians and collectors, and all those having anything to do with manufacturing books and documents.

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THE QUARTERLY REVIEW of BIOLOGY

THE KINGDOMS OF ORGANISMS

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THIS paper proposes the recognition of certain groups of living creatures as kingdoms in addition to the two which are conventionally recognized.

Revision of the primary classification of organisms is a taxonomic operation, governed by taxonomic principles; it differs from revision of a family or genus only in wider scope. Because of the human need of an arrangement of organisms which will express as fully as possible existing knowledge and opinion concerning them, all groups are always subject to revision. Revision is required whenever the currently accepted groups can be replaced by more nearly natural groups. In practice, a group is held to be natural if each of its members is bound to the others by the whole range of its characters, of which some may be common to the whole group, while those in which there is variation exhibit intergradation. Assuming evolution to be a fact, this continuity of character is interpreted as representing, and the quality of naturalness is regarded as identical with, the possession of a common ancestor. Very many of the accepted taxonomic groups are, so far as knowledge can be positive, positively natural. Others are less positively natural; there is an unbroken transition to acknowledged artificialities, groups maintained for the disposition of races too poorly known as yet to permit of their disposition according to relationship.

The ancestor of a particular group, unless it was the original form of life, must also have had ancestors; and from these, if we go back far enough, we will always find collateral descendants. Accordingly, the limits of each natural group are indefinite at one or more points where there are transitions to related groups. We can give broader limits to any group by taking into account a more distant common ancestor, or narrower limits by considering a more recent one. The limits assigned to a particular group, one which is named, assigned to a definite taxonomic category, and defined by description, are always artificial, arbitrary, decided by convenience. Convenience at this point means something subordinate to the overriding convenience or necessity of recog-

nizing groups which are natural. A conservative element of convenience is familiarity: the taxonomist is loath to propose abandonment of a familiar arrangement unless he can propose one in better conformity to relationship. Another element of convenience lies in the varying inclusiveness proper to groups of different categories: phyla and classes should not be too numerous; families and genera should not be too extensive. A third element of convenience lies in feasibility of definition by description; this is often attained by making limits coincide with "missing links," that is, with breaches of knowledge.

The formulation of a system of classification, then, involves a double set of hypotheses: hypotheses as to the ancestry, origin, and evolution of groups, and hypotheses as to what boundaries will be found expedient. A principle useful in the formulation of hypotheses as to the history of groups may here be discussed. If we find that a group of organisms consists of some which possess a certain positive character and of others which do not, we may most often interpret those having the positive character as a subordinate natural group, that is, as being the descendants of a single individual descendant of the ancestor of the greater group. To this principle, however, exceptions are exceedingly common; the same positive character can often be shown to have appeared more than once in evolution, or to have disappeared more than once. So often are we required, by convincing evidence of relationship, to admit to a group members which do not conform to its formal description that this situation may be regarded as the rule rather than the exception.

The application of these principles to the primary classification of organisms will involve breaches of convenience, particularly in the point of familiarity. It is an ancient and familiar hypothesis, too widely accepted as a law of nature, that every living creature is and must be either a plant or an animal. Judged by knowledge and theory which were available to Linnaeus, this hypothesis is sound; judged by modern knowledge and theory, it seems untenable. It was first challenged by Haeckel, whose *Generelle Morphologie* (43; see Fig. 2), in which he proposed the recognition of a third kingdom, Protista, appeared within a decade after the *Origin of Species*. The knowledge by which a tenable rearrangement of the kingdoms could be formulated was not available when Haeckel first attempted it, and although he subsequently, and more than once, rearranged his kingdom Protista, he never won for it a general acceptance. Various authors more recent than Haeckel have shown a disposition to recognize more kingdoms than two, but none of them, apparently, has formulated a system including all organisms. Pending such an accomplishment, the old system of two kingdoms has persisted for want of a workable substitute.

The scientists who find themselves under pressure to devise a more satisfactory system of kingdoms are those charged with elementary instruction in biology or in one of its main branches, as botany or zoology. The elements of the science include the principles of classification, and the teacher is responsible for presenting kingdoms which are limited in accordance with fact and law rather than with tradition. The one who taught me elementary botany made clear to his freshman students the principles of classification; he has summarized them, essentially as above, in various works on the classification of ferns (21, 23). He made it clear that the limits ordinarily assigned to the plant kingdom fail to include groups

which link together the bacteria, the various groups of algae, and the Fungi. Such limits make the kingdom unnatural and are inconsistent with the principles acknowledged. When it became my turn to undertake elementary instruction, my efforts to recognize a series of natural kingdoms led me to distinguish four of them, called Monera, Protista, Plantae, and Animalia. Further reflection and study, extending through about twelve years, have left me confident that this is the best system which can be recognized at the present time. I proceed, therefore, to discuss the four groups as I conceive them, endeavoring to show that each one is acceptable as a unit in a double sense, in evolutionary origin and for purposes of human thought.

THE KINGDOM MONERA

In his *Generelle Morphologie*, Haeckel postulated the existence of a group of organisms without nuclei; he named the group Monera (originally Moneres, but the neuter form used in later works is preferable) and included it in Protista. He is said to have postulated, rather than to have recognized or assembled, such a group, because most of the organisms which he assigned to it, *Protamoeba*, *Protomonas*, and *Vampyrella*, are either nonexistent or false to the definition. Among Haeckel's original examples of Monera, *Vibrio* is the only one representing organisms which actually exist and are interpretable as lacking nuclei.

A few years later, Cohn (19) "with that inspired insight which only unflinching diligence can impart to original genius" (these are the words which Fiske (39) applied to a different scientist and his discoveries) recognized the connection between bacteria and blue-green algae, and combined these organisms in a group which he named Schizophyta ("fission plants"). Earlier scholars (I draw this history from Bergey's *Manual* (5) and from the work of Buchanan (9)) had for the most part regarded bacteria as "animacules," and had given them a place in that group of animals which included the simplest ones and was least definitely defined, namely the Vermes. It may have been the evident relationship of bacteria to blue-green algae that convinced Cohn that they are plants; the group Schizophyta was definitely assigned to the plant kingdom.

Haeckel in his later writings (see his *Wonders of Life* (45)) recognized Cohn's Schizophyta as being the true Monera, and included them, under the latter name, in Protista.

Two authors to my knowledge (22, 106), and doubtless others whom I have overlooked, have published the opinion that the Schizophyta or Monera should be treated as a distinct kingdom. This opinion appears to be correct: I shall present evidence supporting it, but must first discuss the name by which the group is to be called.

Modern usage fixes the application of names by types rather than by descriptions. Under the type system, Monera is the valid name of the group under discussion only if *Vibrio* is recognized as the type, and only if we can attach to the name *Vibrio* some meaning which might have been in Haeckel's mind. The organisms included in *Vibrio* by Mueller, the author of the group, have not been identified. To Haeckel, *Vibrio* seems to have meant bacteria in general. To neither of them could this name have meant the subsequently discovered organism of Asiatic cholera, by which the authors of Bergey's *Manual*, in deviation from their usual nomenclatorial good form, have attempted to typify it. One can perhaps justify Monera as the name of the group

now under discussion by the assumption that to Haeckel *Vibrio* meant *V. subtilis* Ehrenberg (*Bacillus subtilis* (Ehrenberg) Cohn, the type of *Bacillus*). If *Vibrio* is not tenable as type of Monera, or if this name is meaningless, Monera becomes a synonym of Rhizopoda, or, perhaps, loses all meaning. No such ambiguity attaches to the name Schizophyta; it means, and has always meant, bacteria and blue-green algae taken together.

It will be well to take into account certain matters which, under the involved niceties of nomenclature, are not entitled to consideration. The names have connotations: Monera should be organisms without nuclei; Schizophyta should be members of the plant kingdom. As Monera is the older name; as the group is to be treated as a distinct kingdom and distinguished by lack of nuclei; as Schizophyta, applied to a group excluded from the plant kingdom, would be a misnomer and a perpetual annoyance; the name Monera will be used.

The Monera are here treated as a kingdom on the basis of two assumptions: that they are the comparatively little modified descendants of whatever single form of life first appeared on earth, and that they are sharply distinguished from other organisms by the absence of nuclei.

The hypothesis that life came into existence just once is perhaps not absolutely necessary to the treatment of this group as a kingdom. The general rule, that a tenable group is bound together by ancestry, is really a matter of convenience; such a group is bound together by the whole range of its characters rather than by a finite number of specific features. If life originated more than once, it might be expedient to make an exception to the rule by gathering into one group all of the original forms and their comparatively little modified descendants. It is possible that this is done in establishing the kingdom Monera; one prefers to suppose that this group, like any other satisfactory taxonomic group, is natural.

This is a supposition regarding events of an antiquity more easily stated than imagined. Definite fossils from time anterior to the Cambrian, which began, probably, about half a billion years ago (cf. Pirsson and Schuchert (97) and Schuchert and Dunbar (103)) are exceedingly scant. The range of pre-Cambrian time has been divided, tentatively, into two eras; Proterozoic, which commenced, perhaps, about a billion years ago, and Archeozoic, including all preceding time of which the earth harbors objective remains. Great deposits of elemental carbon in the Archeozoic seem to constitute definite proof that life was in existence at least a billion and a half years ago, but there is nothing to show what or how many forms of life were in existence. Life as it exists at present exhibits a deep-seated uniformity, bespeaking a unitary origin: all life resides in mixtures of essentially the same materials; all life obtains the energy for its immediate operations by processes of oxidation; all life (with certain puzzling exceptions among Monera; cf. Crow (27, 28) on *Spirulina*, *Arthrospira*, and other blue-green algae, and Ellis (37) on *Leptothrix ochracea*), exhibits cellular organization. Perhaps, once in existence, life spread over the world and so changed it as to prevent a repeated origin. So ancient is life, so extensive has extinction been, that we would not be convinced of its repeated origin by any amount of negative evidence, such as the absence of all trace of lines of descent connecting the various groups of Monera. On the other hand, the existence of organisms which can be interpreted as the surviving traces of lines of descent would be strong evidence of

unitary origin. I shall endeavor, by a survey of the groups of Monera, to show that such organisms exist.

The hypothesis that bacteria and blue-green algae are without nuclei (and if this hypothesis is false, the name Monera is inappropriate) involves two ideas; one is morphological, concerning the structure of organisms; the other is a matter of words, and concerns the proper use of the term nucleus. Bütschli (10; see also the textbook by Lutman (84) and the long paper by Dobell (34)) is said to be responsible for two distinct conceptions of nuclei in Schizophyta: that the whole cell of ordinary bacteria is a nucleus, and that in sulfur bacteria and blue-green algae there is a central body which is a sort of incipient nucleus, representing, but not showing all of the features of, the nuclei of higher organisms. Dobell reviewed in detail the work of forty-nine previous authors; on the basis of his own work, he concluded that bacteria definitely possess nuclei. In the following survey of the groups of Monera, I shall refer to what is known of the structure of the cells. I must leave it to the judgment of each reader whether any of the structures encountered is to be considered a nucleus.

MONERA: I. AUTOTROPHIC BACTERIA

Most autotrophic (self-nourishing) organisms, including among Monera the blue-green algae, live by photosynthesis. For this process, it is said that a green pigment, chlorophyll, is necessary: more accurately, two green pigments (forms of chlorophyll) are required, and with these there are always associated other pigments, yellow, brown, red, or blue. All these pigments are highly complicated organic compounds. Photosynthesis uses the energy of light, and accomplishes a single immediate result, the production of organic compounds. The energy for all processes except photosynthesis is obtained by processes (collectively called energesis) in which organic compounds are oxidized and destroyed. We cannot suppose that life as it first came into existence possessed substances as complicated as the photosynthetic pigments, nor that it was capable of as complicated a system of metabolism as this.

Still less can organisms which are dependent on others be regarded as primitive. Most of them can be shown to be descendants of organisms which live by photosynthesis; their metabolic system is essentially that of the photosynthetic organisms, but it has been simplified by degradation, by the loss of capacity for the energy-binding process.

Organisms more primitive than those which are photosynthetic or dependent were first discovered by studies of nitrification, that is, of the natural accumulation of nitrates in the surface of the earth. The scientists of the latter part of the nineteenth century were disposed to blame everything on bacteria; several of them attempted to discover nitrifying bacteria. Success in this attempt came to Winogradsky (126).

Only four species of nitrifying bacteria, all discovered by Winogradsky, are known. Some or all of them occur in all soils fit for agriculture; they are of very great economic importance, but it has not seemed worth while to try to control them, and they have been studied but little. Their cells are minute and presumably of the simplest structure. Their system of metabolism is called chemosynthesis: it consists in the oxidation of inorganic compounds, in this case ammonia and nitrites, and the use of the energy released to make organic compounds from carbon dioxide. Thus, in one operation it effects the results both of the photosynthesis and of the energesis of other

organisms. One feels in Winogradsky's original account the bewilderment with which he discovered that the less food he gave his organisms, the better they grew.

Pending a better understanding of the filterable viruses (none of which is known to possess any capacity for making organic compounds from inorganic) the organisms which live by chemosynthesis may be regarded as standing closer to the origin of life than any others yet known. They are, indeed, not very close to the origin of life: they are not intermediate between lifeless matter and living, but are as definitely alive as men. In addition to the nitrifying bacteria, there are other organisms in considerable number which are known or supposed to live by chemosynthesis. Following Bergey's *Manual*, one may treat them as forming three groups (see also Waksman (120)).

Close to the nitrifying bacteria may be placed a sulfur-oxidizing organism of similar character, the *Thiobacillus thioöxidans*, discovered by Waksman and Joffe (121). Here also are placed several genera of obscure organisms which oxidize such substances as hydrogen, carbon monoxide, methane, alcohol, and acetic acid. Several of these are known to be only facultatively autotrophic, and capable of living as saprophytes. These facultatively autotrophic bacteria seem to represent an evolutionary line connecting the purely autotrophic bacteria with the ordinary bacteria of disease and decay.

The order Chlamydobacteria (iron bacteria) includes only about a dozen species (cf. Ellis (37)). Some of these have long been known; the most familiar is *Leptothrix ochracea*, which forms the yellow masses by which we recognize the presence of iron in springs of water. Since the discovery of chemosynthesis, it has been supposed that the iron bacteria live by oxidizing ferrous iron to ferric, but this has apparently not been positively proved.

The order Thiobacteria is something of a miscellany; the characters are the accumulation within the cells of granules of sulfur or of salts of calcium or both, or the possession of a red pigment, or both such granules and such pigment. *Beggiatoa*, a colorless inhabitant of sulfur springs, forming filaments which exhibit a writhing movement, has long been known. Winogradsky showed that it lives by oxidation of hydrogen sulfide and elemental sulfur. Gardner (40) described the protoplasm of *Beggiatoa* as forming a network in which a central body is distinguished by greater coarseness of the strands, and apparently also by staining reactions (the preparations which he figured (Fig. 1, q) do not show the latter character). The pigmented Thiobacteria, the "purple bacteria," are, at least in part, saprophytic. They have the property of swimming toward light; it is suspected that they can to some extent use the energy of light, and that they represent a stage in the evolution of photosynthesis.

This is not the proper occasion for putting forward a new taxonomic system of Monera—that would only divert attention from my proper thesis. I have in mind, however, and have been following, a tentative outline which may as well be stated explicitly. The Monera seem not numerous enough for classification in groups of seven ranks as prescribed by the botanical and zoological codes. The category of phyla may be omitted, and the main groups of Monera treated as classes. The groups already described as including organisms which live by chemosynthesis may form a single class of three orders. The remaining Monera may form three classes, embracing respectively the ordinary bacteria or Schizomycetes (orders Eubacteria, Actinomycetes, and Myxobacteria), the spirochaets (a single order), and the blue-green algae (two or three orders).

MONERA: II. ORDINARY BACTERIA

One and only one apparent evolutionary line has been pointed out as leading from autotrophic bacteria into the group here called ordinary bacteria. The latter is a numerous group of parasites and saprophytes, many of which are familiar and of great importance. As the character of the group—physiological dependence—is negative and evidently derived, one can have no confidence that the group is natural; further study may show how to break it up.

The structure of ordinary bacteria is simple. A complete list of the morphological characters in which there are variations usable in classification would include few beyond the following: size and shape of cells; absence or presence and pattern of flagella; production or non-production of spores, gelatinous envelopes, and involution forms; a few staining reactions; characters of colonies. The Myxobacteria, an insignificant group of curiosities, produce comparatively complicated structures which may perhaps be interpreted as highly elaborated colonies.

In physiological characters, as distinguished from morphological, the ordinary bacteria exhibit a remarkable range of variations; the classification is largely based on these.

A note on the position of the nitrogen-fixing bacteria may be included here. They form three distinct groups. One, the species *Clostridium butyricum*, is in Bergey's *Manual* duly placed among ordinary bacteria. The other two, the genera *Rhizobium* and *Azotobacter*, are placed near the nitrifying bacteria. Nitrifying bacteria and nitrogen-fixing bacteria agree in being Monera and in being concerned with the nitrogen cycle. In all other respects they differ; nitrogen fixation is an endothermic process found only in parasites and saprophytes. Places for *Rhizobium* and *Azotobacter*—two different places—should be found among ordinary bacteria.

Among the numerous papers on the internal structure of cells of ordinary bacteria I cite but few. Schaudinn (101) described the exceptionally large *Bacillus Bütschlii* found in the gut of the cockroach (Fig. 1, d–g). He finds the protoplasm finely alveolar and divisible into a central body and an outer part.

When spores are to be formed, a spiral row of granules appears at the outer edge of the central body. This row breaks at the middle, each part migrates to the end of the cell nearer to it, and is involved in the formation of a spore. Each cell, accordingly, forms two spores. In *Bacillus Sporonema*, a smaller organism found as a free-living saprophyte, Schaudinn (102) was unable to find the structures just described. Swellengrebel (111) worked on *Bacillus maximus buccalis* from his own mouth (Fig. 1, a–c). He describes a peripheral spiral filament, which, in each cell division, divides lengthwise, after which the parts separate by sliding past one another as a smaller spring may be pulled out from within a larger one. Swellengrebel's figures support this surprising account, but there are no other reports to confirm it. Dobell (33) described several bacteria from the guts of frogs and toads; among these, *Bacillus flexilis* shows stages quite like those of *B. Bütschlii*, and like it produced two spores from each cell. In *Bacillus Saccobranchi*, which he discovered in the blood of a dead fish (34), he similarly found stainable material appearing either as separate granules or as a crooked, more or less spiral rod (Fig. 1, h–n). The granules or rod, as the case may be, stain as chromatin does.

It may be noted that with the exception of those of Swellengrebel, the results just summarized are drawn entirely from spore-forming rods, the group which forms the genus *Bacillus* as properly construed. Comparatively recent work on this group tends to confirm these results. According to Churchman (57), the outer part of the protoplast is different from the inner; the gram-positive character of the group depends on the outer part. The figures of *Bacillus subtilis* by Knasyi (69) and of *B. Megatherium* by Bayne-Jones and Petrilli (3) seem to show the spiral bodies of the older authors as thickenings of the ectoplasm (this is Knasyi's term; it is preferable to Churchman's "cortex"). These bodies were not found, however, to have

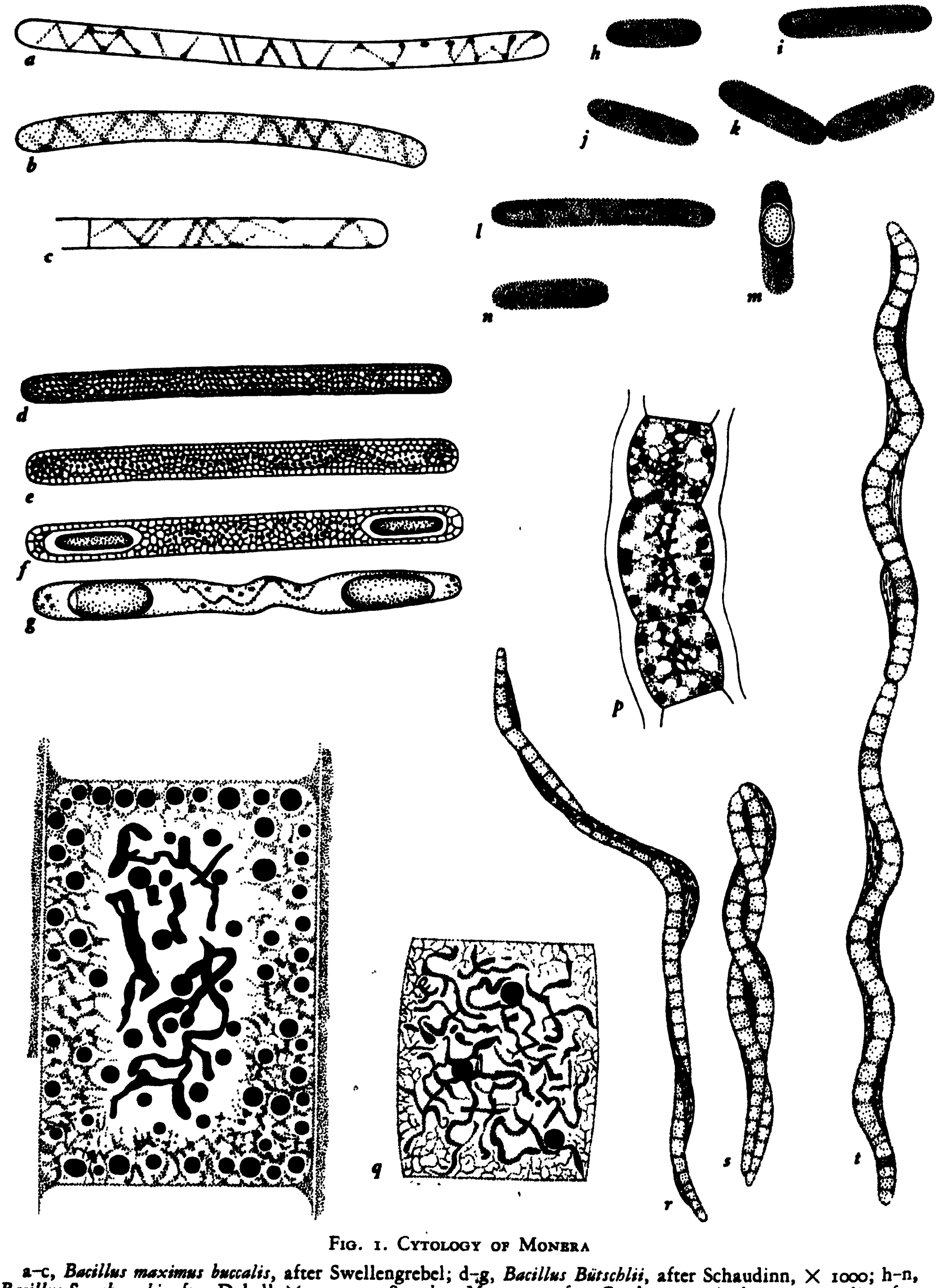

FIG. 1. CYTOLOGY OF MONERA

a–c, *Bacillus maximus buccalis*, after Swellengrebel; d–g, *Bacillus Bütschlii*, after Schaudinn, × 1000; h–n, *Bacillus Saccobranchi*, after Dobell, × 2000; o, *Symploca Muscorum*, after Gardner; p, *Anabaena circinnalis*, after upt, × 2000; q, *Beggiatoa archnoidea*, after Gardner; r–t, *Cristispira Veneris*, after Dobell, × 2000.

an active part in cell division or in spore formation.

MONERA: III. SPIROCHAETS

Spirochaets (the genus *Spirochaeta* and its allies) were first distinguished by Ehrenberg. They include small forms which may be saprophytes in waters, commensals in the alimentary tract of animals (as in the mouth of man and the gut of termites), or parasites in blood, and larger forms of which the most familiar are parasites in mollusks. The group became an object of intense study when Schaudinn showed that one of the species is the cause of syphilis. The character of the group is a spiral body which is flexible. It is a small group, but is so distinct that it must be treated as one of the main groups of Monera.

The cytology is most definitely known in the large forms from mollusks; I describe it primarily from Dobell's account of *Cristispira Veneris* (35; Fig. 1, r-t). The body bears a longitudinal membrane. Internally, it is divided into a series of chambers; if one compares it with filaments of *Spirulina* and *Arthrospira*, one may become uncertain whether it is the whole body or each separate chamber that is to be regarded as a cell. At the margin of each septum between chambers, there is a whorl of granules which stain like chromatin; Dobell interprets these, collectively, as a nucleus. Cell division is transverse; but there is a stage of division during which the two parts of the dividing cell lie side by side; as the free ends separate, they give a false appearance of lengthwise division.

It has been suggested that the spirochaets are related to the trypanosomes, which also inhabit blood and have flexible bodies and lengthwise membranes. This hypothesis served the proper function of hypotheses, that of stimulating investigation, but it should have been abandoned by 1910, when it had become evident that the resemblance extends to no features beyond the ones just stated.

MONERA: IV. BLUE-GREEN ALGAE

Blue-green algae (Myxophyceae Stizenberger 1860; Cyanophyceae Sachs 1874; Schizophyceae Cohn 1879; cf. Setchell and Gardner 104)) are the Monera which possess chlorophyll and live by photosynthesis. They are as common as dirt; they have long been known, but were only gradually recognized, during the latter half of the nineteenth century, as being quite distinct from green algae. As the group is distinguished by positive characters, it is probably natural. It is apparently very ancient; certain species now living build calcareous masses in hot springs, and calcareous masses of similar character are known from the Proterozoic and Archeozoic. I have shown reason, however, for regarding blue-green algae as less primitive than the nitrifying and sulfur oxidizing organisms, and for supposing that they are descended from the latter through the purple bacteria.

The following account of the cytology of blue-green algae is based primarily on the work of Haupt (52; see Fig. 1, p), with consideration of the older accounts of Bütschli (10), Gardner (40; see Fig. 1, o), and Swellengrebel (112).

When cells are studied without sectioning, a central body is evident; sectioning, however, shows that the finely vacuolate protoplasm is uniform throughout the cell. The outer part is distinguished by the presence of pigment in the vacuoles, the inner by the presence of rods and granules, staining like chromatin, imbedded in the protoplasm. The inner part contains also granules which stain red with methylene blue. Gardner calls these "α-granules". They are evidently the same as the "red granules of Bütschli" or metachromatic granules known also from *Beggiatoa*, many ordinary bacteria, and various other organisms. As the cell divides by constriction, the inner part is divided; the rods and granules may be divided, and are distributed at random to the daughter cells.

The above evidence may justify the position (tentative as all scientific con-

clusions are, yet maintained with confidence) that bacteria and blue-green algae are a natural group, being the forms in which life, since its origin, has undergone least change; that they are distinguishable by lack of nuclei; and that they should be treated taxonomically as a kingdom named Monera.

NUCLEATE ORGANISMS

In all organisms except the Monera, the life of every cell is conditioned by the presence within it of one or more nuclei. The nucleus is a part of the protoplast set apart (at least when it is not dividing) by a membrane. Its most definite character is the process, mitosis or karyokinesis, by which it divides into two. During this process a part of the contents (the chromatin) becomes organized as a definite number of definite bodies called chromosomes, each of which is divided into two parts which are distributed respectively to the two daughter nuclei.

Occasionally, nuclei are found to divide by constriction, without going through the mitotic process. Nuclei formed by definitely non-mitotic divisions are unable to persist without limit; sooner or later, such nuclei always decompose and disappear. Non-mitotic division, either binary or multiple (in the latter form called "formation of chromidia") was formerly supposed to be the normal process in various unicellular organisms, especially certain rhizopods. This has been disproved by Kofoid (70) and his associates.

Typical and durable nuclei can originate not only by division, but also by certain fusions of nuclei, always of just two nuclei, which can differ in their heredity only in minor details. Such fusion is the essential feature of sexual reproduction. It gives rise to diploid stages, stages in which each nucleus has a double set of chromosomes; a life cycle in which it occurs must also exhibit at some point a modification of mitosis called reduction division, or meiosis, in which the chromosomes separate into two groups without splitting lengthwise, so that the original or haploid chromosome number is restored. For reasons which are not clear, the reduction division is usually associated with one or more other nuclear divisions which seem to be essentially ordinary mitotic divisions. In the great majority of nucleate organisms, reduction division is followed by just one other division, so that the whole process yields four haploid nuclei.

The uniformity of mitosis; and, if one denies sexual reproduction as a primitive function of the nucleus, then the capacity of the nucleus to assume identical sexual behavior in groups as diverse as men and diatoms, wheat and wheat rust; furnish evidence that the nucleus has come into existence only once in evolution; that all nucleate organisms are related and constitute a natural group, a super-kingdom. The oldest known remains of nucleate organisms are from the late Proterozoic; they represent Radiolaria, sponges, Foraminifera, and even, apparently, worms. None of these groups can be regarded as including the original form of nucleate life. In attempting to date the origin of the nucleus, one must allow time for the evolution of these groups, perhaps the full length of the Proterozoic, back to a billion years ago. No remains of nucleate organisms older than the groups just mentioned are to be expected. Knowledge of the origin and early evolution of nucleate life must be obtained, if it can be obtained at all, by study of races which survive.

Among living organisms, the overwhelming majority of macroscopic forms are properly listed, on the basis of relationship, in the kingdoms of plants and animals. Among microscopic organisms likewise many, as rotifers, nematodes, and

green algae, are legitimate plants or animals. But many microscopic, and a number of macroscopic, organisms fall into groups which cannot confidently be assumed to be descended from any form which would properly be regarded as either a plant or an animal. One of the objects of this paper is to show that these groups should be treated as an additional kingdom: that they form, if taken together, a natural group, having the original nucleate organism as a common ancestor; and that it is more convenient to maintain this group as a taxonomic entity than to make certain other arrangements which would also be consistent with natural classification.

THE APPLICATION OF THE NAME PROTISTA

The groups which, as here proposed, are to constitute a separate kingdom are those which zoologists treat as Protozoa together with the diatoms, the marine algae, and the Fungi. Before they are considered in detail, it will be expedient to show that Protista is the proper name for the combined group.

Protista is the oldest name after Plantae and Animalia to be published as that of a kingdom. As already mentioned, it was published by Haeckel in his *Generelle Morphologie*, in 1866. The views on classification presented in this work were summarized in a figure which is here reproduced (Fig. 2). The figure is a phylogenetic tree; in fact, it is the original phylogenetic tree, of which all others are modifications. Haeckel was the first to use this familiar device for representing the relationships of taxonomic groups.

The figure is seen to represent three kingdoms. The animal kingdom is arranged as it was understood at the time, except that the sponges and the unicellular "animacules" have been excluded; the Infusoria, however, are not yet recognized as unicellular and are placed among the worms. In the plant kingdom, the new name Archephyta is coined for Chlorophyceae, among which, according to the knowledge of the time, the blue-green algae are included. Red and brown algae are present; so are the Fungi, combined with Lichenes under the name Inophyta. At the summit of the plant kingdom stand the bryophytes and vascular plants, arranged in quite modern fashion.

In the third kingdom, Protista, are the flagellates with *Noctiluca* sharply separated from the others; the diatoms; and the sponges, to which the specialist on marine life has devoted a space out of proportion to their significance. Sporozoa are represented by the gregarines, which are included with certain other organisms in a group called Protoplasta. Aside from these groups, practically all of Haeckel's original Protista are or have subsequently been included in Rhizopoda. This is true of the Myxomycetes; of the Protoplasta, excepting the gregarines; and of the Monera, excepting *Vibrio*.

For the purpose of applying the name Protista, it is desirable to recognize a nomenclatorial type. As we ascend the tree, the first name encountered is that of *Vibrio*, the representative of the bacteria. Since it is clear that Haeckel was but poorly acquainted with bacteria, we would be anchoring the name Protista in a fashion which he could not have intended if we should select *Vibrio* as the type. I think that we may safely select as type of Protista the zoologists' standard example of a rhizopod, the organism commonly known as *Amoeba Proteus* Leidy (for an unusually fine example of the nomenclatorial tangle which can be woven about a familiar species, see the references to this species in the papers of Boeck and Stiles (7) and Schaeffer (100)).

Haeckel's life work subsequent to the

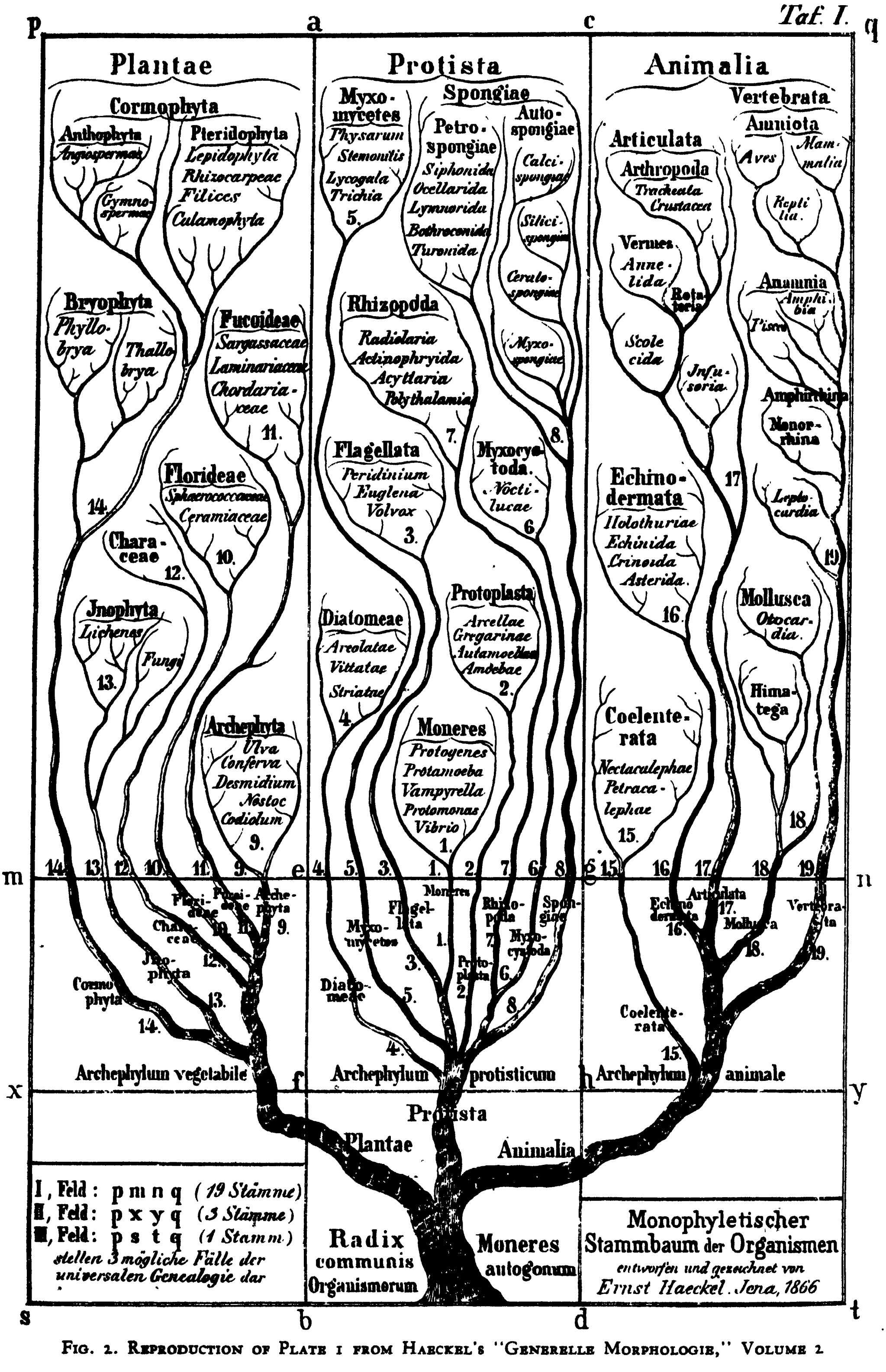

Fig. 2. Reproduction of Plate I from Haeckel's "Generelle Morphologie," Volume 2

publication of the *Generelle Morphologie* consisted in large part in elaborating and refining the ideas expressed in the phylogenetic tree just discussed. In this original tree, he avoided expressing an opinion as to whether life had originated just once or many times: the three transverse lines in the lower part of the figure indicate three levels at which life might have come into existence. He was sufficiently aware of the significance of the nucleus to set apart a group characterized by its absence; he was not sufficiently aware of it to avoid suggesting that life might repeatedly have come into existence equipped with nuclei. In his *History of Creation* (44) he suggested the multiple origin of life and the development of nuclei in several distinct lines. In his late work *The Wonders of Life* (45) he avoided the question by presenting a manifestly artificial "morphological classification" essentially as in the attached table (Table 1).

TABLE 1

Haeckel's "Morphological Classification" (1904)

	"KINGDOM" PROTISTA, (UNICELLULAR ORGANISMS)		"KINGDOM" HISTONA, (MULTICELLULAR ORGANISMS)
	Monera, organisms without nuclei	Nucleate Protista	
Plants	Blue-green algae	Green flagellates, diatoms, etc.	Metaphyta
Animals	Bacteria	Rhizopoda, Infusoria, etc.	Metazoa

By the history just sketched, the name Protista came to have several connotations in the minds, apparently, both of the one author who maintained the group and the many who rejected it. It was conceived as the group representing the most ancient forms of life; the group distinguished by the unicellular character; the group which lies between plants and animals; the group which is essentially a combination of Monera and Protozoa. The use of nomenclatorial types enables one to overlook the connotations, the conflicting supposed definitions, of the name of a group until after the application of the name is determined. If *Amoeba Proteus* is the type of Protista, then whatever kingdom aside from Plantae or Animalia includes this species must be called Protista. On many occasions the results of applying the type system seem outrageous to established conventional usage, but I do not think this will be found true of the present case. The name Protista is here applied to a group considerably amended since it was set up for flagellates, rhizopods, diatoms, and sponges, but the amendments are no greater than one would expect as a result of seventy years' advance in knowledge of the groups concerned. Most of the original Protista retain their place in the kingdom; the group continues to include the common ancestors of plants and animals; it has the common characters of plants and animals in the nucleus and features dependent on the nucleus; it consists chiefly of unicellar organisms. This application of the name Protista to a particular group is qualified, however, by the condition that the group can be justified as natural and convenient.

PROTISTA: I. PIGMENTED FLAGELLATES

Justification of the group involves an enumeration of the subsidiary groups, with a consideration of the origin and characters of each. This survey should begin with the most primitive of nucleate

organisms. We cannot as yet recognize, either among living organisms or among fossils, a series connecting Monera with nucleate organisms, nor any very limited race which was the first to possess a nucleus. We can, however, reconstruct to a considerable extent the characters of the original nucleate organism and we can place it in a certain major group of existing organisms.

The original nucleate organism must have been unicellular rather than multicellular, and autotrophic rather than dependent. All autotrophic nucleate organisms live by photosynthesis. As photosynthesis occurs in certain Monera, we may be confident that this function was inherited from Monera by the original nucleate organism. The photosynthetic pigments in nucleate organisms are always confined to certain organelles called plastids. These do not occur in Monera; they were evidently evolved more or less concurrently with the nucleus.

Nucleate unicellular organisms living by photosynthesis are included in the natural groups (commonly construed as orders) called chrysomonads, Heterokontae, cryptomonads, dinoflagellates, chloromonads, and euglenids. (Other organisms with these characters are included among diatoms and green algae; these two groups are evidently derived, and need not be considered in the present connection.)

In the six groups just listed, the typical members are motile by means of flagella. Several if not all of them include, however, forms which lack either or both the characters of flagellation and pigmentation; there are colorless flagellate forms, amoeboid forms, and stationary forms which may be either unicellular, colonial, or filamentous. The amoeboid character appears to be an adaptation for holozoic nutrition, that is, for the ingestion of solid food. It is almost always associated with loss of pigmentation and is obviously a derived condition. The colorless flagellate forms and the colonies and filaments are likewise obviously derived. As to whether possession of flagella is a primitive character in these groups, we may reach a conclusion by considering the alternatives, that flagella of essentially identical character (there are differences in detail, as pointed out by Deflandre (32)) have been developed independently in all six groups, or, on the other hand, that they are inherited from a common ancestor of all six groups, and that the non-motile forms are derived. The latter alternative is surely the sound one. Fagella, like the function photosynthesis, appear to be an inheritance from Monera, and to have been characters of the first organisms that developed nuclei and plastids.

It appears, then, that all organisms which are at the same time nucleate, unicellular, flagellate, and capable of photosynthesis constitute a natural group. It is, however, not expedient to recognize a taxonomic group limited by these characters. In the taxonomic system, as we have seen, organisms as just described are distributed among six groups (or, counting green algae, seven), and to each of these groups are admitted organisms lacking flagella or photosynthetic pigments, or forming bodies of more than one cell. These groups are distinguished by differences in the pattern of flagellation, in the particular pigments present, in storage products, in materials and structure of walls or shells, and in other features. It has been possible only to a very limited extent to show that some of them are derived from others. The chrysomonads are generally supposed to be the most primitive, and Pascher (93, 96) has shown that the Heterokontae (also the diatoms) are related to, and presumably derived from, these. It is evident from the isolation of these groups that they are very ancient, and some authorities have been disposed to raise them to very high taxonomic rank. It is also evident, however, that the six groups taken together are still a natural group, being essentially the sub-class Phytomastigina of zoologists, the division Chrysophyceae of Tilden

(115). All other nucleate organisms may be regarded as derived from this group.

In dealing further with the evolution and groups of nucleate organisms, features of the nucleus will be found significant. Although essentially uniform, the nucleus varies in details; the variations fall into more or less parallel evolutionary lines in the various groups. For the sake of having a definite conception of the starting point of these lines, we need much more knowledge than we have of the nuclei of the pigmented flagellates. In particular, the supposedly primitive chrysomonads are poorly known.

There have been a considerable number of accounts of nuclear structure and behavior among the euglenids. Early observations of *Euglena* revealed a large intranuclear body which divides during mitosis and seems to lead the chromosomes in separating. This body was designated a nucleolo-centrosome; we may for the present call it by the term endosome, which implies an internal body without specifying its nature. Modern work on *Euglena agilis* by Baker (2) and on *Euglena Spirogyra* by Ratcliffe (98; Fig. 3) has shown that the endosome, some time before nuclear division, buds off a body which moves to just within the nuclear membrane and divides.

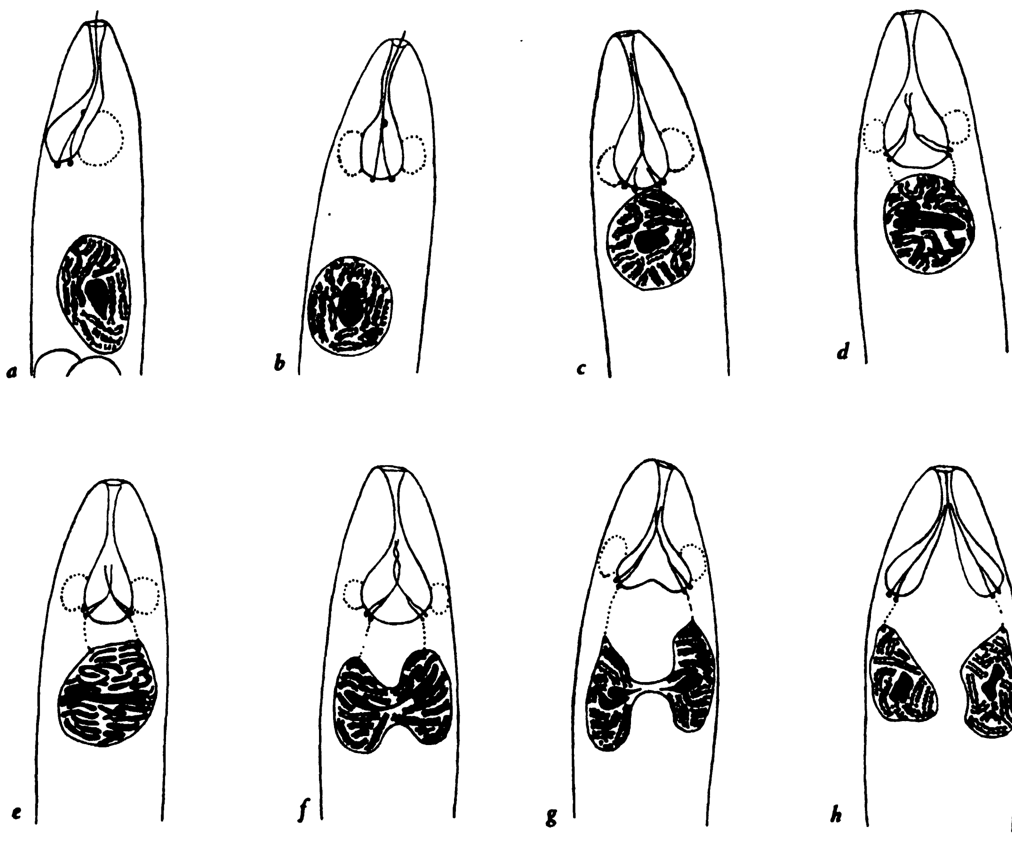

FIG. 3. NUCLEAR DIVISION IN EUGLENA SPIROGYRA, AFTER RATCLIFFE, × 720

The term centrosome may be applied to this body and to the two bodies formed by its division rather than to the endosome. As division begins, the nucleus moves forward within the cell and comes into

contact with the cell membrane in the bottom of a depression at the forward end. Each centrosome seems to generate, just within the adjacent cell membrane, a body called a blepharoplast; the nucleus then withdraws from the cell membrane, leaving the centrosomes connected to the blepharoplasts through fibers called rhizoplasts. In *Euglena Spirogyra* (the details are different in *E. agilis*) the flagellum, which is attached within the reservoir and already forked at the base, splits lengthwise; a new flagellum grows out from each blepharoplast and becomes fused, not far from the base, with one of the halves of the old one. Meanwhile, within the intact nuclear membrane, the chromosomes and endosome are dividing. The centrosomes are at the sides of the dividing nucleus, not at the poles of a spindle; no spindle has been recognized. Nuclear division is completed by the constriction of the membrane. Subsequently, the centrosomes and rhizoplasts disappear, to be replaced during the next division by new ones.

In the euglenids *Menoidium*, studied by Hall (46), and *Peranema*, studied by Hall and Powell (49) the centrosomes are permanent bodies which divide before the nucleus does and stand at the poles of the dividing nucleus. The dinoflagellates *Oxyrrhis* and *Ceratium* were also studied by Hall (47, 48) and found to agree in general with *Menoidium*, although *Ceratium* lacks the dividing endosome. Hall and Powell are unwilling to accept the features in which *Euglena* is supposed to be different from *Menoidium* and these other genera but the essential agreement of the results of Baker and Ratcliffe is evidence of their accuracy.

Tentatively, in view of the scant data considered, I am disposed to take the system of nucleus and accessory structures in *Euglena* as the most primitive yet known. The chromatin and chromosomes are essentially as in all other nucleate organisms; in the many millions of years since the origin of the nucleus, the chromatin of different organisms has acquired the power of transmitting a bewildering variety of hereditary qualities, but the only visible changes have been fluctuating variations in its arrangement in the resting nucleus, and in the number, size, and shape of chromosomes. The centrosome would appear to be originally a device for the production of flagella, related to mitosis only in that mitosis makes new flagella necessary. The endosome, although it gives rise to centrosomes, is not in itself a centrosome or a nucleolo-centrosome, but it may yet be a nucleolus. The nuclear membrane and nuclear sap, which disappear during division in the nuclei of higher organisms, are here permanent structures, persisting through division and being divided, and the endosome may be a nucleolus with corresponding qualities. Its original significance may be as a guide to the separating chromosomes of organisms so primitive as not to have developed a spindle. It may continue to persist through mitosis after centrosomes have come to occupy the poles of the dividing nucleus, as in *Menoidium* and *Oxyrrhis*, but in *Ceratium*, and, in fact, in all organisms except a very few, it is either absent at all times or disappears during division. The spindle seems to have originated subsequently to polar centrosomes.

PROTISTA: II. ANIMAL-LIKE FLAGELLATES

There exist many flagellates beside those included in the six groups just considered. All these others are dependent (holozoic, saprophytic, or parasitic); they have been arranged in four orders which together form a subclass Zoomastigina.

A few genera whose members are amoeboid constitute the order Pantostomatida.

The Protomonadida, with one or two flagella to each cell, are a varied assemblage. Here are included the Monadidae, the genera of which, as Pascher (94, 95) has shown, would be naturally placed by distributing them among the chrysomonads. Here also are included the trypanosomes—parasites in the blood stream of animals, the most intensively studied of all flagellates—and the choanoflagellates or collared monads, of interest because cells of similar structure are an element in the bodies of sponges.

The Polymastigida have three to eight

flagella per cell (per nucleus, in certain races with multinucleate cells); the Hypermastigida have more than eight flagella per cell. The members of these groups are largely entozoic; the Hypermastigida are confined to insects, particularly termites. They are notable for elaboration of the systems of structures (neuromotor systems) which extend from the nuclei to the flagella and other parts of the cell. Cytologically, as a result of the work of Kofoid and his associates at the University of California, these are the best understood of flagellates.

Kirby's (67) reorganization of the polymastigote family Trichomonadidae gives a convenient view of some of the parts which may make up a neuromotor system, together with some hints as to their evolution.

There is usually a permanent centrosome located just outside the nuclear membrane. Typically, this is connected by a rhizoplast to a blepharoplast or a cluster of blepharoplasts standing near the cell membrane. In *Trichomitus Termitidis* a single body is regarded as a combination of centrosome and blepharoplasts, although one might suspect that by homology it represents only one of them, the other being suppressed. The Hypermastigida, incidentally, although not directly related to *Trichomitus*, resemble this form in having a centroblepharoplast. The blepharoplast or centroblepharoplast, as the case may be, bears several free flagella together with an internal rod which is called the axostyle and is suspected of being homologous with a flagellum. Laterally attached to the blepharoplasts there is in some examples a darkly-staining mass called a parabasal body. One flagellum is usually reversed, trailing behind the cell as it swims; in *Trichomonas* and its immediate allies, this is grown fast to the cell, forming an undulating membrane. Concurrent with the evolution of the undulating membrane has been the evolution of an internal rod attached to a blepharoplast and serving apparently as a mechanical support to the undulating membrane. Kirby, distinguishing this from both the axostyle and the parabasal body, names it the costa.

The features of mitosis in Polymastigida and Hypermastigida may be illustrated by a comparatively simple, and in this respect presumably primitive example, namely, *Trichomonas buccalis* as studied by Hinshaw (54; see Fig. 4). The centrosome, blepharoplast, and rhizoplast divide; the centrosomes remain connected to one another, for

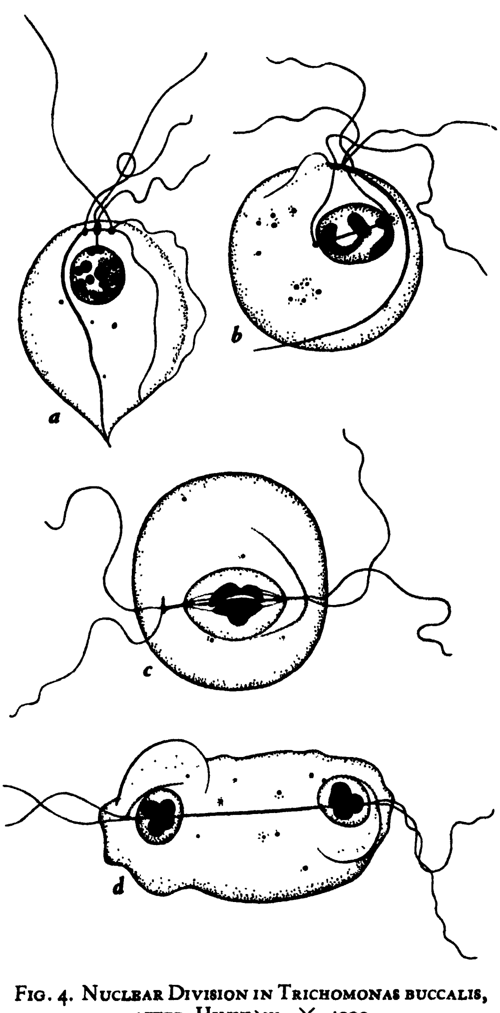

FIG. 4. NUCLEAR DIVISION IN TRICHOMONAS BUCCALIS, AFTER HINSHAW, × 4000

a time, by a fiber called the paradesmose. There is no dividing endosome. A spindle is formed within the nuclear membrane between the centrosomes. The nuclear membrane does not disappear; after the chromosomes have separated, it divides

by constriction. The free flagella are divided between the two daughter cells, each of which regenerates the parts of a complete cell which it has not inherited.

Although showing much variation, and in some examples much elaboration, the mitotic process in most Polymastigida and all Hypermastigida is essentially as in *Trichomonas buccalis*.

See, for example, the work of Kofoid and Swezy on *Trichomitus Termitidis* (72), *Chilomastix Mesnili* (75), *Giardia enterica* (77), *Trichonympha Campanula* (73), and *Trichonympha (Leidyopsis) sphaerica* (74); also that of Kirby on *Dinenympha fimbriata* (62), and *Staurojoenina assimilis* (63). A minority of the polymastigotes, including the Oxymonadidae studied by Kirby (65) and Connell (20), and *Streblomastix Strix*, studied by Kidder (61), show deviations profound enough to seem significant of a different evolutionary origin. All, however, have a permanent nuclear membrane, dividing by constriction; and in all in which a spindle is present it is inside the nuclear membrane.

Since many colorless flagellates have been found to belong to typically pigmented groups, it is not to be supposed that the Zoomastigina, being merely the ones not yet so placed, constitute a natural group. The diversity of the group and of its three orders, aside from Hypermastigida, confirms the suspicion that these groups are artificial. The complete breaking up of these groups, by the discovery of the relationships of their members, will be an arduous task, and pending this accomplishment, the groups will have to be allowed a place in the taxonomic system.

PROTISTA: III. DIATOMS

The diatoms are a numerous group of unicellular (less commonly colonial or filamentous) organisms with brown plastids. They have finely, elaborately, and characteristically sculptured shells of silica; the shell of each cell consists of two parts fitting over each other, as the textbooks say, like the parts of a pill-box. The cells may be non-motile, or motile by means of flowing bands of protoplasm which function like endless belts.

The existence of sexual reproduction in the groups just treated as flagellates is questionable; in diatoms it is positively established. Reduction division takes place immediately before the sexual fusion of nuclei; this means that all nuclei of diatoms, except those formed for the purpose of sexual fusion, are diploid. This is a character of groups in which sexual reproduction is ancient and presumably inherited from pre-existing groups; it strengthens the impression made by the other characters of diatoms, that this is a derived, highly specialized group.

Nuclear division in diatoms is best known by the old work of Lauterborn (83). His results, puzzling in many details, were confirmed in most respects by Karsten (58). There is a centrosome at the nuclear membrane (apparently outside). This buds off a ring-shaped structure which enters into the nucleus and by growth becomes a tube extending clear through it. The nuclear membrane disappears early in mitosis, but the nuclear sap remains for a time distinct from the cytoplasm. The chromosomes gather in a mass at the middle of the tubular structure; they then divide into two doughnut-shaped masses which travel to the ends of it. As these masses become organized into new nuclei, the cytoplasm seems to absorb the nuclear sap, and likewise the tubular structure, but not until the latter has budded off a new centrosome from each end.

Lauterborn and Karsten interpreted the tubular structure, in terms of the knowledge of their time, as a central spindle. It seems possible, however, that this structure represents a chromatin-dividing appa-

ratus more ancient than any sort of spindle; perhaps it is homologous with the dividing endosome of the euglenids. Pascher, as already mentioned, has shown that the diatoms are related to the chrysomonads. A confident interpretation of the mitotic process just described will depend on an understanding, yet to be obtained, of the chrysomonads.

The brown algae (Melanophyceae Stizenberger 1860; Phaeophyceae Kjellmann 1891; cf. Setchell and Gardner (104)) show an evolution from filamentous forms with a life cycle of similar haploid and diploid stages to thalloid forms with a considerable differentiation of organs and tissues, and whose haploid stages are reduced to the mere gametes. They produce

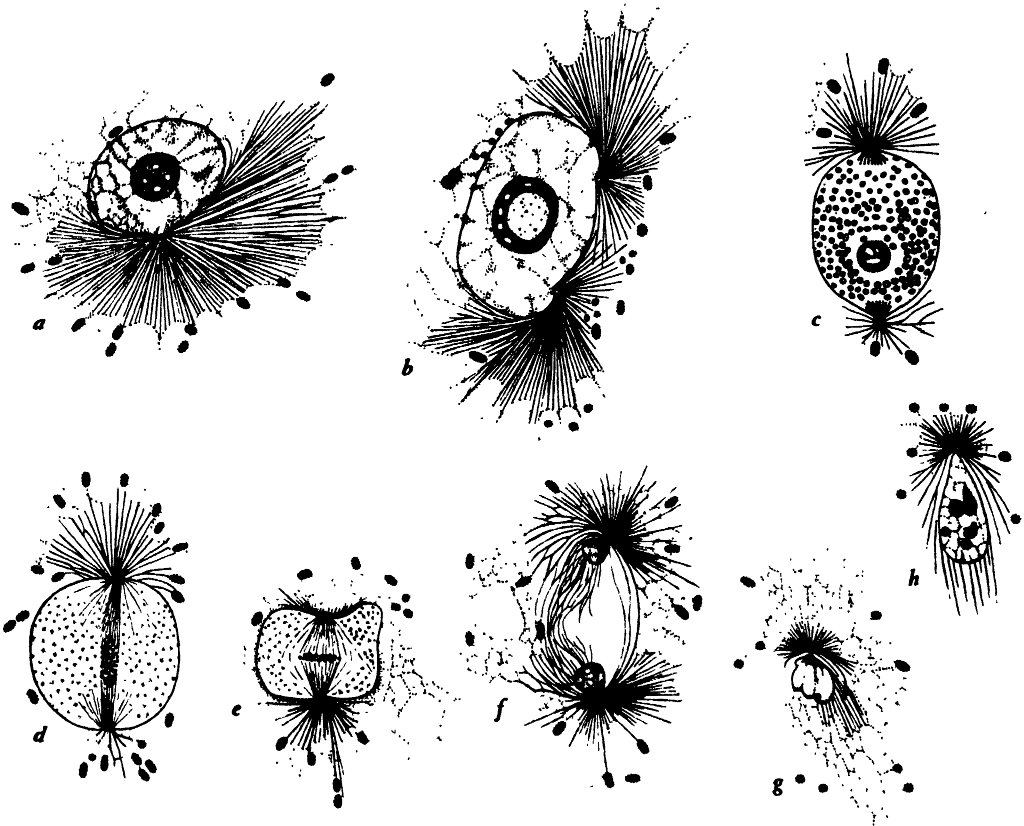

FIG. 5. NUCLEAR DIVISION IN STYPOCAULON, AFTER SWINGLE, × 800

PROTISTA: IV, V. MARINE ALGAE

The two great groups of marine algae consist of multicellular organisms, some of which are exceedingly large; they live by photosynthesis and have plastids of other colors than green; they produce no true starch or cellulose. Beyond these characters, they have little in common; they are not closely related.

flagellate reproductive cells, each bearing two laterally attached unequal flagella; these cells are so small that the presence of any structures connecting the nuclei with the flagella is doubtful.

In Sphacelariaceae, among which Swingle (113) studied particularly *Stypocaulon* (see Fig. 5) there is a permanent centrosome located just outside the membrane of

each nucleus. It stands at the focus of an aster, a mass of fibers radiating into the cytoplasm; such a structure is found in many groups, including diatoms but apparently not flagellates. Before each nuclear division, the centrosome and aster divide into two, which move apart along the membrane to opposite sides of the nucleus. A spindle forms within the nuclear membrane; it appears as if it were projected from the two centrosomes. The nuclear membrane persists nearly until the end of the mitotic process. Then, as the two groups of chromosomes begin to become organized as daughter nuclei, the distinction between nuclear sap inside the membrane and cytoplasm outside disappears, and the membrane becomes invisible. New membranes form about the daughter nuclei.

The Sphacelariaceae are comparatively primitive brown algae. In other brown algae—*Dictyota* as studied by Mottier (91); *Fucus* and *Cutleria*, by Yamanouchi (129, 130); *Zonaria*, by Haupt (53); *Pterygophora*, by McKay (89)— the centrosomes and asters are absent when the nuclei are not dividing; they appear *de novo* as mitosis commences, and disappear at the end. As an exception, the centrosomes formed during the first or true reducing division in *Dictyota* persist, divide, and function during the immediately following second division. In brown algae in general the nuclear membrane disappears earlier in the mitotic process than it does in *Stypocaulon*.

The descent of brown algae from flagellates is evident. The possession of centrosomes is probably related to the production of flagellate reproductive cells. But those who have studied the groups most closely are unwilling to connect the brown algae with any particular group of flagellates.

The red algae, Rhodophyceae, include certain poorly known organisms of comparatively simple organization; but most of the very large number of species are of complicated structure and exhibit complicated reproductive processes. They produce no motile cells whatever. The cell walls are of a pectinaceous material which appears in commerce in the diverse forms of agar-agar and edible birds' nests.

Features of mitosis in this group were first described by Davis (29), who studied *Corallina;* his results were confirmed and extended by the work of Yamanouchi (127, 128) on *Polysiphonia*.

During most mitoses, centrosomes appear outside of the nuclear membrane at the poles of the intranuclear spindle. There are no asters. As mitosis proceeds, the centrosomes swell, become less stainable, and finally disappear. The old nuclear membrane disappears during the later stages of nuclear division; new ones are formed about the daughter nuclei. The second division of the reduction process, although supposedly essentially an ordinary mitotic process, shows in *Polysiphonia* as in many other organisms certain peculiarities in detail. At the end of the first, or proper, reduction division, the nuclear membrane does not disappear, and does not divide by constriction. No centrosomes appear for the second division, which, taking place within the intact original nuclear membrane, results in four groups of chromosomes in a single tetrahedrally lobed space. The membrane continues to persist where these clumps of chromosomes are against it, but dissolves in the areas between, so that each of the four new nuclei has a membrane which is partly new and partly inherited.

I take the temporary centrosomes of this group to be vestigial structures, indicative of a flagellate ancestry, but there is nothing to connect the red algae with any particular group of flagellates; they are an advanced and highly isolated group.

PROTISTA: VI, VII. RHIZOPODA AND SPOROZOA

The Rhizopoda are nucleate organisms with exposed protoplasm which can be thrust forth in projections called pseudopods. As authority for the name, Siebold,

1845, is cited; Sarcodina of Hertwig and Loesser, 1874 (cf. Stiles and Hassall (110)), was intended to apply to a larger group including this, but must be regarded as a synonym. These organisms were formerly regarded as representative of the starting point of life, and hence as being automatically a natural group. The positive evidence for these views, in the supposed formation of nuclei *de novo* from chromidia, has been discredited; and the evidently derived character of the few known self-nourishing organisms which are amoeboid is convincing evidence to the contrary. Amoeboid forms with flagella are placed naturally among the chrysomonads (*Chrysapsis*) and Heterokontae (*Chloramoeba*); others are conventionally stationed among the Zoomastigina (*Mastigamoeba*) and Sarcodina (*Naegleria*, *Trimastigamoeba*). It will be convenient to call this sort of organisms collectively the amoebo-flagellate complex. Many recognizable natural groups seem to be descended from the amoebo-flagellate complex, and among them are several which are assigned to Rhizopoda; but the Rhizopoda taken together are clearly an artificiality. As various lines of rhizopods have come into existence by loss of characters from organisms which were themselves simple, it will not be easy to find characters indicating their respective true relationships; the group will have to be maintained for some time to come.

The groups included here as orders are Lobosa, Foraminifera, Heliozoa, Radiolaria, and Myxomycetes. All of them appear to be natural except the first. The Foraminifera and Radiolaria, having shells suitable for preservation as fossils, are known to be very ancient, as we might expect in groups having no assignable nucleate ancestors except flagellates.

The best known cytologically, of Rhizopoda as of Zoomastigina, are the entozoic species studied by Kofoid and his associates. The scientists of the California school were not the first to describe a normal mitotic process in amoebas, but in a long series of papers (16, 60, 64, 71, 76, 78, 79, 80, 124) they have shown that typical mitosis is typical of the group. In the resting nucleus, the chromatin is largely or entirely gathered into a single mass called a karyosome. A centrosome, the only remnant of a neuromotor apparatus, is found during mitosis just within the persistent nuclear membrane; it divides, and the parts remain connected by a fiber which, being within the nucleus, is called an intradesmose. A spindle is present; Child (16; see Fig. 6), working on *Endamoeba gingivalis*, found that it forms before the centrosome divides, extending from the centrosome in among the chromosomes which have formed from the karyosome; later, as the two daughter centrosomes move apart along the nuclear membrane, it opens like a jack-knife opening, to form a straight line.

The following are some of the observed chromosome numbers:

Councilmania Decumani	4
C. dissimilis	8
C. Lafleuri	8
C. Muris	6
Endamoeba coli	6
E. dysenteriae	6
E. disparita	12
E. gingivalis	6

In the "*Vahlkampfia* group" the chromosomes are smaller and more numerous, and there are prominent polar caps of stainable material within the dividing nucleus. These polar caps, mistaken for separating masses of chromatin, are largely responsible for the reports of non-mitotic division in amoebas. Within these caps, Kofoid and Swezy (79) first discovered centrosomes in *Karyamoebina falcata*.

In the amoebo-flagellate *Naegleria*,

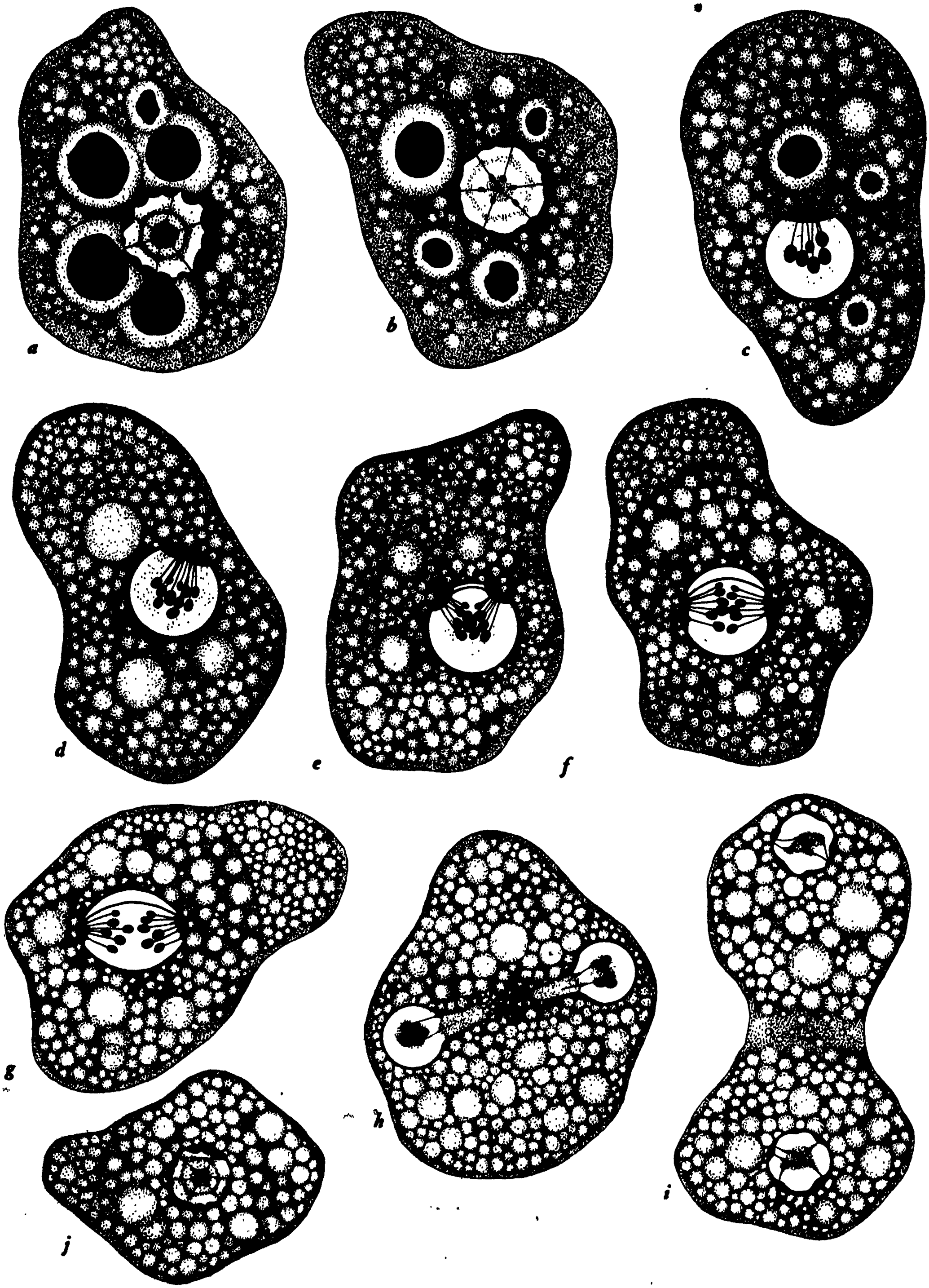

Fig. 6. Nuclear and Cell Division in Endamoeba gingivalis, Original Figures by H. J. Child, × 3750

which is normally amoeboid but readily induced to form a flagellum, Wilson (124) found that mitosis is of the *Vahlkampfia* type. When the flagellum is present, a rhizoplast connects the blepharoplast to the intranuclear centrosome, and another connects this to the karyosome. The extra-nuclear neuromotor apparatus—all of the neuromotor apparatus except the centrosome—is discarded or absorbed before division; it is regenerated from the centrosome, and Wilson was disposed to believe that this emerges from the karyosome.

The Myxomycetes will be considered separately.

The Sporozoa will merely be mentioned. The group includes very many species, all parasitic in animals. They have complicated life cycles, involving sexual reproduction; they are ordinarily non-motile, but have flagellate or amoeboid stages, indicating descent from the amoeboflagellate complex. They are apparently not a natural group. *Monocystis* was found by Calkins and Bowling (12) to have an extra-nuclear centrosome. This divides during the early stages of mitosis, and a spindle is formed between the daughter centrosomes; the nuclear membrane dissolves, and the spindle is carried laterally in among the chromosomes. The process is very much as in animals.

PROTISTA: VI A. MYXOMYCETES

The Myxomycetes were so named, as a group of Fungi, by Link, 1833. Some twenty-five years later de Bary, recognizing their resemblance to rhizopods, named the Mycetozoa (cf. Macbride and Martin (85)). In their vegetative condition, Myxomycetes are colossal amoebas adapted to life in air; the plasmodium, as the vegetative stage is called, is a naked mass of protoplasm containing thousands of nuclei, moving pseudopodially, and nourishing itself in holozoic fashion. Plasmodia are inconspicuous only because they keep to damp and shaded habitats. In reproduction, the protoplasm builds more or less elaborate structures by secreting pillars, fibers, and walls, of lifeless material, among which it undergoes cleavage into little spores homologous with the cysts of other rhizopods. The germinating spores release amoeboid cells which presently develop flagella.

The nuclei are minute, and the authors who have attempted to study them have found difficulty in recognizing nuclear division in the plasmodium. Just previous to spore formation, and again when the spores are germinating, mitoses are recognizable (see Harper (51) on *Fuligo;* Jahn (56) on *Stemonitis;* Howard (55) on *Physarum;* Gilbert (42) on *Ceratiomyxa*). A sharp-pointed spindle is formed within the nuclear membrane, and definite centrosomes have been recognized at the poles. The nuclear membrane persists for some time, but disappears before the end of mitosis. A nucleolus is present in the resting nucleus, and fades out at about the same time as the nuclear membrane. The nuclear divisions just before spore formation are supposed normally to include a reduction division; in *Ceratiomyxa*, a genus distinguished from other Myxomycetes by a variety of characters, reduction division takes place within the spore, which accordingly becomes 4-nucleate.

Gilbert gives a full description of the formation of flagella in *Ceratiomyxa*.

> The 4-nucleate protoplast escapes from the spore wall; after it has undergone various changes in form, each nucleus divides. While the protoplast is dividing by constriction into eight, each nucleus comes into contact with the cell membrane. The part of each nucleus which comes into contact with the membrane is the part diametrically away from the sister nucleus formed by the preceding division; this is the part where a centrosome, retained since the preceding division, may be presumed to be present. From the

point of contact, a flagellum is suddenly projected beyond the cell membrane; the nucleus then withdraws from the cell membrane and is found to be connected to a blepharoplast at the base of the flagellum by something which looks like a double rhizoplast. The accounts of enflagellation in *Stemonitis* and *Physarum* represent essentially the same process; but Jahn interpreted the structure between the nucleus and the base of the flagellum as a conical region of clear cytoplasm, whose boundaries would appear in optical section as two fibers.

The flagellate cells, in many Myxomycetes if not in all, are gametes; they fuse in pairs. The Myxomycetes are diploid in all stages except spores and gametes. It is not certain whether the amoeboid zygotes can combine with each other in forming the plasmodium, or whether each plasmodium is developed from a single zygote.

The Myxomycetes are evidently a natural group, and are evidently descended from something in the amoebo-flagellate complex.

PROTISTA: VIII. FUNGI

The group called Fungi, as here construed, consists of parasites and saprophytes whose bodies consist of filaments with rigid walls of chitin. They may be arranged in four classes. Two classes, Basidiomycetes and Ascomycetes, are highly developed groups, numerous in species, evidently natural, and showing in their characters some relationship to each other. Zygomycetes are a comparatively small and primitive group, not connected to the others by any forms confidently recognizable as intermediate.

The remaining class, the one usually and properly listed first, is Oomycetes. It embraces a variety of forms so broad that one cannot be positive either that the group is natural or that it is not. The main body of the class, consisting of the orders Saprolegniales and Peronosporales, is a natural group of typical filamentous fungi. They resemble in many respects such green algae as *Vaucheria;* the resemblance is usually taken as indicative of relationship, but is open to interpretation as being a result of parallel evolution.

The order Chytridiales, also included in Oomycetes, has been used as a dumping ground for poorly understood parasites. Here have been placed the intracellular parasites of flagellates, Infusoria, and rhizopods (see Becker (4), Campbell (13), Connell (20), Kirby (66, 68), Kofoid (70), Sassuchin (99)). Some of these, at least, are obvious bacteria. Leaving such things aside, the chytrids can be recognized as having a character of their own. A protoplast, originally naked and flagellate, makes its way into a cell of an alga or higher plant. There it develops a rigid wall, and usually sends out filamentous branches. It becomes multinucleate, and eventually breaks up into naked swimming cells which escape, usually, through a walled tube. Such an organism can be interpreted as a link between the amoebo-flagellate complex and the typically fungal Saprolegniales and Peronosporales.

Mitosis is known in several genera of the latter orders (see Davis (31) on *Saprolegnia;* Couch (26) on *Leptolegnia;* Davis (30) and Stevens (107, 108) on *Albugo;* Stevens (109) on *Sclerospora*). It resembles that of Myxomycetes: the sharp-pointed spindle, at the ends of which centrosomes have been detected, is formed within the nuclear membrane. The membrane persists until about the middle of the mitotic process. The nucleolus is rather persistent, and Stevens has in some cases found it to divide into two parts which pass to the poles of the spindle.

Cotner (24, 25) has described the origin of flagella on the swimming cells of several genera. The nucleus is drawn out into a beak which reaches, or nearly reaches, the cell membrane. From the

beak, the one or two flagella (the number is constant in each genus) are projected; the nucleus then withdraws from the surface of the cell, but remains connected to the blepharoplasts at the bases of the flagella by one or two rhizoplasts. All this is quite as in *Ceratiomyxa*.

The mitotic processes of Oomycetes have been described chiefly from the reproductive structures; the vegetative nuclei in the filaments are too small, and are not easily enough found in division, for convenient study. The same situation holds in Ascomycetes and Basidiomycetes. The three nuclear divisions, including the reduction division, which lead to spore formation in Ascomycetes have been described by a long series of authors, who consistently confirm Harper's (50) early account of the process as observed in *Erysiphe* (Fig. 7). A centrosome lying next to the nuclear membrane, apparently fused to its outer surface, develops an aster toward the outside and a bundle of spindle fibers toward the inside. The centrosome divides, and as the two daughter centrosomes move apart along the nuclear membrane, the ends of the two parts of the spindle swing apart until they form a straight line. The nucleolus disappears; the nuclear membrane persists until the later stages of mitosis, when it seems to dissolve or collapse, leaving each cluster of chromosomes, while shredding out into a network, to develop a new membrane. The centrosomes persist, dividing at each nuclear division; at the end of the last nuclear division, the nuclei thrust them forth on beaks. These beaks, since the spores are dispersed in air, do not generate flagella; instead, they seem to induce the formation of a spore wall some distance out from and surrounding each nucleus.

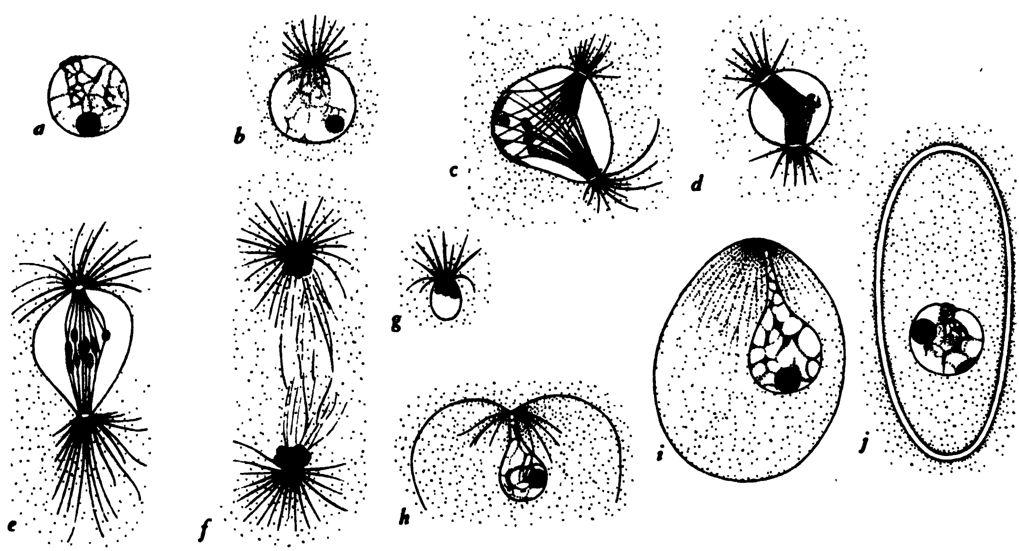

FIG. 7. NUCLEAR DIVISION AND FORMATION OF ASCOSPORES IN ERYSIPHE COMMUNIS, AFTER HARPER

Many studies of Basidiomycetes, of such diverse groups as mushrooms, puffballs, and rusts, have been carried out with the use of technique refined enough to yield chromosome numbers, which are apparently always two or four. The spindle is

developed within the nuclear membrane, and is sharp-pointed; Lander's (82) figures of *Scleroderma* show definite centrosomes at the poles. The nuclear membrane seems always to disappear early in the mitotic process, though the nuclear sap may remain distinct from the cytoplasm for some time longer.

The spore-producing organ, the basidium, includes one original nucleus which undergoes a reduction process of two divisions.

In *Coprinus*, Vokes (119) describes the nucleus as moving up to the cell wall; when it withdraws, four points on the wall are found to be connected to one point on the nuclear membrane, where we can imagine a centrosome, by four fibers, possibly homologous with rhizoplasts. The point of attachment seems to divide as nuclear division begins. The spindle is formed within the nuclear membrane, with the points of attachment as poles; the membrane presently disappears, but there are two fibers attached to each pole of the spindle, and, subsequently, to one point on the membrane of each daughter nucleus. The second division goes forward in much the same manner as the first; each of the four resulting nuclei has one fiber attached to it. As the four nuclei are formed, the cell wall grows out, at each point where a fiber is attached, and forms a little cavity at the end of a slender tube; each nucleus moves up the fiber attached to it and into one of the cavities. Each of the resulting bodies—the wall of the cavity and the contained cytoplasm and nucleus—is cut off as a spore.

It was long ago suggested that the Fungi are not a natural group; that the Ascomycetes may be placed near the red algae, and the Oomycetes and Zygomycetes broken up and distributed near various groups of green algae. Traces of this arrangement remain in the recent classification of Fungi by Clements and Shear (18). Gäumann (41) accepts most of the Fungi as a natural group derived through Saprolegniales from green algae, but derives some of the chytrids from the rhizopods. Martin (87) maintains that the whole range of Fungi, including Myxomycetes, is a natural group. The evidence seems to me not strongly con firmatory of any of these views. For the present it will be convenient to assume that Martin's view is correct; that Fungi proper and Myxomycetes represent parallel lines of development from more or less the same member of the amoebo-flagellate complex. Under this assumption, natural classification would permit the treatment of Myxomycetes either as a group of Fungi or as a separate group. The more convenient alternative is the treatment of Myxomycetes as a separate group, or their assignment to Rhizopoda, since this facilitates the descriptive definition of the group to which the name Fungi is applied.

PROTISTA: IX. INFUSORIA

Infusoria are distinguished by the possession of cilia, structures typically shorter in proportion to the size of the body than flagella, more numerous, and distributed generally over the surface. The Infusoria reach fairly large sizes, and may be individually visible to the naked eye; they are common, numerous in species, and familiar, and are notable for an elaboration of the structure of the individual cell exceeding that of other organisms. A mouth and gullet (more technically cytostome and cytopharynx) are adaptations for holozoic nutrition. The bases of the cilia are linked together by an elaborate neuromotor apparatus; this does not, however, come into contact with the nuclei. In most Infusoria there are two kinds of nuclei, both represented in every cell; in other words, each cell contains at least two nuclei which are not alike.

Nuclei of the more conspicuous kind, called macronuclei, divide by a non-mitotic process; and, at intervals, they dissolve and disappear, to be replaced by new ones originating by the division of micronuclei. This process is called endomixis. There is a sexual process in which pairs of cells form a junction without losing their individuality. The macronuclei dis-

solve, while the micronuclei undergo three or four divisions including a reduction process (since reduction takes place just before sexual fusion, all nuclei except those capable of fusion are diploid). Each cell receives one of the nuclei formed by reduction division in the other; this unites with one of those formed by reduction division in its own body; the remaining haploid nuclei degenerate and disappear. The cells separate, and in each of them the fusion nucleus divides once or more than once; macronuclei and micronuclei arise by the differentiation of the nuclei formed by these divisions. It is evident that only micronuclei retain the genetic powers of proper nuclei.

It has long been known that micronuclei divide mitotically, and that their membranes do not disappear but undergo constriction at the end of the process. Turner (117), working on *Euplotes Patella*, discovered in the micronucleus an endosome which divides during mitosis, the parts remaining connected for some time by a fiber. The daughter endosomes seem to accompany, rather than to lead, the separating chromosomes; they may be comparable with the endosomes of *Euglena* rather than with centrosomes.

The Infusoria are evidently a natural group. Like red algae, they are a highly evolved group of unknown origin, and an evolutionary blind alley which has led to nothing higher.

PROTISTA: DISCUSSION

A great number of nucleate organisms have now been surveyed under the assumption that they constitute a kingdom Protista. They have been arranged in nine groups which may be construed as phyla or divisions. Some of these are manifestly artificial, but I have tried to show that the whole assemblage is a natural group, that all of these organisms are derived from the one original nucleate organism by lines of descent which lie entirely within the divisions considered (see Fig. 8). If the whole assemblage is a natural group, the question of recognizing it as a taxonomic group, a kingdom, is one of convenience.

One element of convenience, as already mentioned, lies in the feasibility of definition by description. As the Protista are separated from Monera by a broad evolutionary gap, it is easy to distinguish them from Monera by a character, namely, the presence of nuclei. From plants and animals, Protista can to some extent be distinguished by primitive features of the nucleus. The apparently most primitive of known nuclei have a membrane which does not disappear during mitosis, but divides by constriction. Centrosomes, and spindles formed within the intact nuclear membrane, are features of very primitive, if not of the most primitive nuclei. We may regard the permanent nuclear membrane, the centrosome, and the intranuclear spindle, as the positive characters of typical Protista. All these characters fade out in the evolution of various lines: we find the nuclear membrane disappearing at earlier and earlier stages in brown algae and in Fungi; the centrosome, permanent in the lowest brown algae, is present only during mitosis in the higher brown algae; is absent during the second division of the reduction process in *Polysiphonia;* has not been detected in many Basidiomycetes. The spindle originates in the cytoplasm of the sporozoan *Monocystis*. And, while the characters of typical Protista are absent from the higher Protista, they are to some extent present, as would be expected, in the lowest plants and animals.

The kingdoms of plants and animals, being derived groups, are distinguished respectively by combinations of positive characters pecular to themselves. The Protista may be distinguished by the absence of these characters, but it is to be remembered that organisms can be retained as plants or animals even if by degenera-

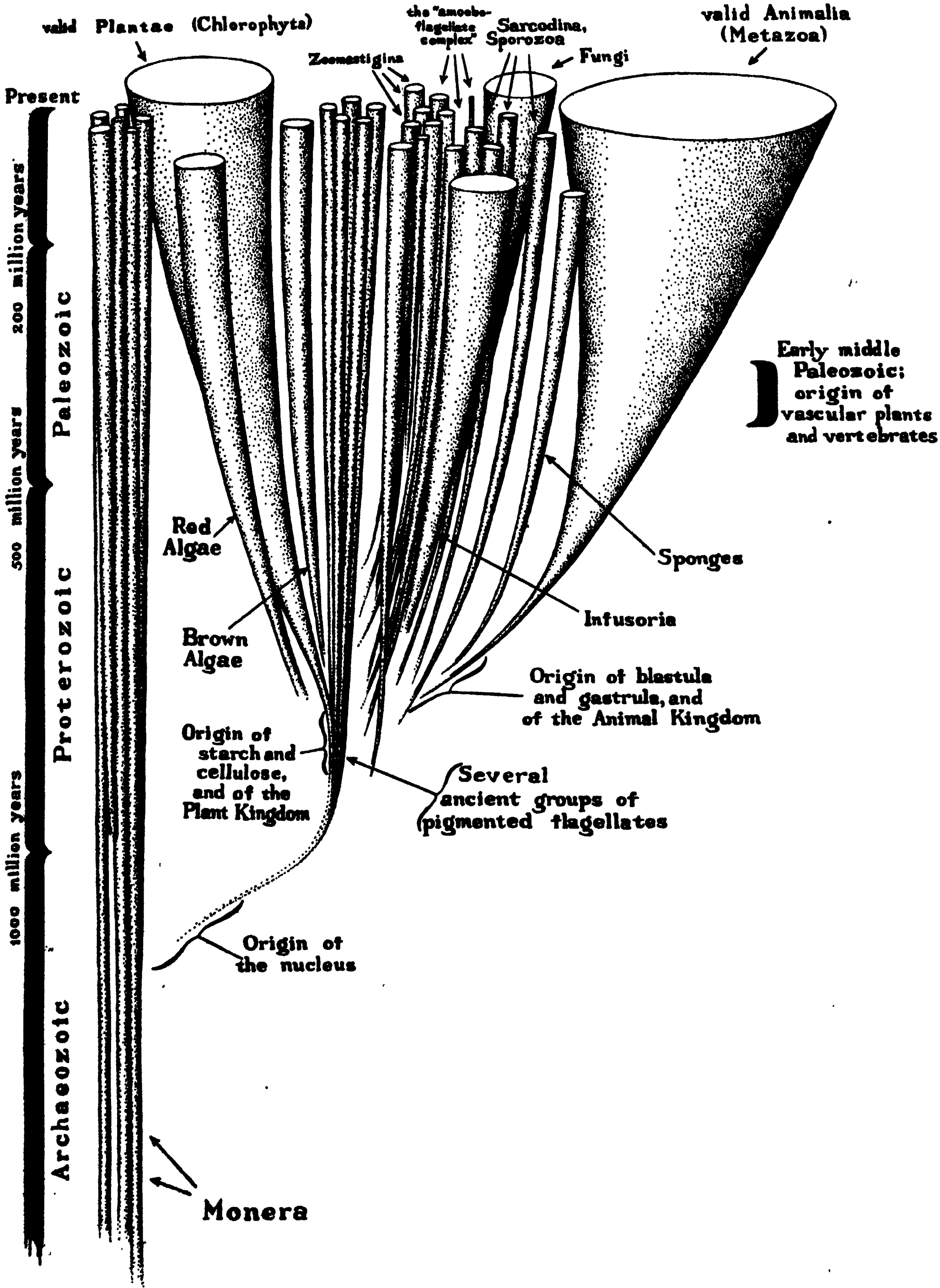

FIG. 8. DIAGRAM OF THE GENERAL PHYLOGENY OF ORGANISMS AS PRESENTED IN THE PRESENT PAPER

tion they lose the characters of the kingdoms to which they belong. Ultimately, it is not by characters but by relationship that groups are defined, and the more extensive the group, the more numerous will be the exceptions to the formal descriptive characters.

We may with equanimity abandon the attempt to define Protista by characters, positive or negative, which will not admit of exceptions. The convenience of the group will appear most definitely by contrast with alternative dispositions of the included organisms.

The traditional disposition of these organisms has been by partition (apparently with a subconscious attempt at equity) between plants and animals. The plant kingdom resulting from this partition, as presented in the text books, includes a group Algae, divided into classes distinguished by pigmentation, as blue-green, green, brown, or red, and a group Fungi, including bacteria and Myxomycetes. It is obvious that by this treatment neither Algae nor Fungi nor the plant kingdom is a natural group. Toward the end of the nineteenth century, Engler and Prantl (38) attempted to correct this situation by annexing the whole group of flagellates. Not all botanists welcomed this proposal; Thaxter (114), an outstanding authority on Myxobacteria, Zygomycetes, and Laboulbeniales (organisms having little enough in common with proper plants) characterized the annexed group as "a menagerie of organisms whose zoology is orthodox to a degree." The real objection in natural classification to the Englerian system is not the orthodox zoology of the flagellates, but the appearance that annexations were not carried far enough. In order to link the Fungi and Myxomycetes into the plant kingdom, we need also the Rhizopoda, and as the annexation of this group leaves the Infusoria and Sporozoa at loose ends in the animal kingdom, we might as well take these groups with the others. This is, indeed, not the only way of setting up a natural system of two kingdoms: the zoologists can keep the Protozoa in their kingdom, if they are willing to accept along with them the Monera, the diatoms, the marine algae, and the Fungi. These groups move as a block; an equitable partition which is at the same time natural is, to present knowledge, an impossibility.

Recognizing the extreme inconvenience of throwing the whole range of Protista (and the Monera along with them) into either Plantae or Animalia, and recognizing also the impossibility of distributing these organisms between the two kingdoms, some authors have proposed to recognize, in place of the one kingdom Protista here described, a series of several kingdoms. This is, for example, the position of Smith (106), who, having distinguished six phyla of algae, remarked that "in reality, the six divisions listed above represent six kingdoms. Five of these kingdoms would have but one division each." Similarly, such a treatment might be satisfactory to Martin (87), who remarked that "Myxomycetes, Phycomycetes, Ascomycetes, and Basidiomycetes . . . together constitute a phylum, to be included among plants as a matter of convenience, but in reality neither plants nor animals, but an independent group of organisms, one of several such." This multiplication of kingdoms is not in itself inconsistent with natural classification, but neither now nor in immediate prospect would it be found practical, in such a multiplication of kingdoms, to make them all natural. A system granting regnal rank to such petty groups as cryptomonads, and to such artificialities as Zoomastigina and Sporozoa, is scarcely desirable. Natural classification permits

all these groups to be treated as one unit, and it is more convenient to do so than to treat them as a dozen or more units.

The balance of authority has been strongly against the recognition of a kingdom Protista. The objection advanced with most show of reason states that the line between plants and animals is recognized with difficulty, and that the establishment of a group placed between them will increase the difficulty by requiring the recognition of two vague lines instead of one. Here I may introduce an analogy. It seems that mankind, for the most part, consists of three races. Imagine that by immemorial tradition mankind has been held to consist of two races: the line between them will be a constant source of difficulty, but the difficulty will become inconsiderable whenever science is persuaded to recognize two lines instead of one. It will presently be shown, of the kingdoms left as plants and animals by the exclusion of Protista, that each of these groups can be defined by positive characters to which exceptions are reasonably few, and that each kingdom is quite definitely limited by its characters to certain subordinate groups. The situation to which objection is made is imaginary.

In undertaking actually to use Protista as a taxonomic entity, I recognize a difficulty which did not concern the authors who refused to do so. Organisms previously within the jurisdiction of two different nomenclatorial codes are to be placed in a group for which no code has been framed: the result may be nomenclatorial confusion. We need not take this difficulty too seriously. The art of nomenclature rests as much on antiquarian as on biological science. We have seen enough nomenclatorial confusion, in the realms governed by codes, to know that biology, and even taxonomy, can survive it. Meanwhile, I have taken advantage of the escape from codes to apply consistently the practice of Linnaeus in capitalizing all specific epithets which are proper nouns.

PLANTS

Such is the concept of Protista, as it has just been formulated, that an account of the characters and limits of Plantae and Animalia will complete the characterization and limitation of Protista, and will, in fact, complete the limitation of kingdoms which is the object of this paper.

The limits of the plant kingdom are those which will include the two groups Chlorophyceae (green algae) and Embryophya (higher plants). The positive characters are the possession of chloroplasts, that is, of plastids containing the four pigments Chlorophyll A, Chlorophyll B, carotin, and xanthophyll (and no others), and the production of two specific carbohydrates, true starch and true cellulose. Some of these characters appear to some extent among Protista. The plastids of Heterokontae, chloromonads, and euglenids can scarcely be called anything but chloroplasts, though they may differ from those of proper plants in the relative abundance of the different pigments. The carbohydrates starch and cellulose have been reported from various Protista. Blackman's (6) account of the dinoflagellate genus *Pyrocystis* refers to a cellulose which does not give a blue color with zinc chlor-iodide, and to a starch which does not give a blue color with iodine. Maltaux and Massart (86) refer without qualification to starch as occurring in the cryptomonad *Chilomonas*. There are other such reports, and it is not improbable that some of them are correct. But no organisms except proper plants show the complete combination of plant characters.

The lowest group in the plant kingdom

as here construed is the order Volvocales. In an evolutionary sense, this group and its descendants, as distinguished from all other organisms, are plants. The Volvocales have the characters of flagellates, and are by zoologists regularly listed as the order Phytomonadida of class Mastigophora. This disposition of the group is in quite as good accord with natural classification as the botanical treatment which places the Volvocales among green algae: we have here a perfect example of an evolutionary link between two groups. The botanical treatment is followed here as being the more convenient, in emphasizing the positive characters, the chloroplasts, starch, and cellulose, of the Volvocales.

We have in Kater's (59) account of *Chlamydomonas* a thoroughly satisfactory description of the nucleus and mitosis of a primitive and typical example of the Volvocales. With this as a starting point, we can make out the course of the evolution of the nucleus in plants: a matter which is of interest as tending to confirm the interpretation of the evolution of the nucleus already given in connection with Protista.

Chlamydomonas (Fig. 9) has a neuromotor system of two flagella, a blepharoplast, a rhizoplast, and an intranuclear centrosome.

During mitosis, all of these are cast off or dissolved except the centrosome; the nucleolus also dissolves, as in higher plants. The dividing centrosome forms an intradesmose, and a spindle forms within the nuclear membrane with the daughter centrosomes as poles. The nuclear membrane persists until mitosis is nearly complete, but eventually dissolves instead of undergoing constriction. The neuromotor systems of the daughter cells are formed as outgrowths from the centrosomes. The whole process is of great interest as being intermediate between what we observe in flagellates and what we observe in higher plants. As in most flagellates, but not the primitive euglenids, the centrosomes have become division centers and the nucleolus dissolves instead of dividing. There has possibly been a stage resembling what we find in the trichomonad flagellates, in which the neuromotor system and nuclear membrane are permanent, being divided and inherited at each division. The dissolution of these parts seems to be a matter of degeneracy of the neuromotor system, but it is a preliminary to advance in the evolution of the organism as a whole.

The Volvocales, with several other orders, belong to the class Isokontae. This is the most significant of the classes of green algae, as being the most numerous in species, and as including both the most primitive green algae and those which come closest to the higher plants. No other Isokontae seem to be as well understood, cytologically, as *Chlamydomonas*. From several genera (see Allen (1) on *Coleochaete;* Timberlake (116) on *Hydrodictyon;* von Cholnocky (17) on *Ulothrix*) there have been reports of granules at the poles of the mitotic spindle; these may be recognized as centrosomes. They are found most usually during the divisions preceding the formation of flagellum-bearing swimming cells.

Among green algae belonging to minor groups—classes apparently derived from Isokontae and leading to nothing further—the genus *Spirogyra* has been the most studied. There is a recent precise account of the behavior of the nuclear membrane and spindle during division by McAllister (88).

The spindle appears first in polar positions outside the nuclear membrane; subsequently it extends within, not breaking through the membrane in any crudely mechanical fashion, but being extended beyond it. The part first formed, outside the membrane, becomes invisible while the part inside persists and functions. The nucleus becomes football-shaped; the membrane remains intact for some time, but eventually, as in *Chalmydomonas*, it disappears, and new membranes are formed about the daughter nuclei. No trace of centrosomes remains; this is perhaps associated with the complete absence of flagellum-bearing cells in the group represented by *Spirogyra*.

It is possible for an organism to retain the occasional habit of producing flagellum-bearing cells after losing all trace of centrosomes, though this situation is not usual. It is exemplified by *Oedogonium*, which represents another evolutionary side-line among green algae. In this genus, Ohashi (92) finds that there is no centrosome. The nuclear membrane disappears early in the mitotic process. When a cell is to develop flagella, a number of granules appear *de novo* in the cytoplasm. They move to the cell membrane and arrange themselves in a ring where the flagella are to form.

FIG. 9. NUCLEAR AND CELL DIVISION IN CHLAMYDOMONAS NASUTA, AFTER KATER, × 2567

As we turn from green algae to higher plants, we find that many of the latter produce male gametes which are motile

by means of flagella. In liverworts, mosses, and some of the fern allies, the sperms are biflagellate, apparently as an inheritance from the Isokontae; in ferns, cycads, and the maidenhair tree, they bear many flagella, apparently as a modification of the biflagellate condition. The flagella born by sperms of these higher plants arise always from granules which stand at the poles of the spindle during the mitoses by which the sperm nuclei are formed.

There is a considerable literature (cf. Sharp (105)) concerning these granules; they may be absent during all mitoses except those by which the sperm nuclei are formed, or may appear during a few previous mitoses; or, in liverworts, there may be traces of them elsewhere in the body (see Chamberlain (15) on *Pellia* and Van Hook (118) on *Marchantia*). Following Chamberlain (14) and Sharp, we may accept these bodies as being centrosomes, though not all authorities have done so. The additional term blepharoplast, coined by Webber (122), has been found useful in dealing with organisms whose neuromotor apparatus includes a flagellum-bearing structure distinct from the centrosome.

In the highest plants, the conifers and the angiosperms, there are no flagellate cells whatever. There are no traces of centrosomes; nuclear membranes disappear at the beginning of mitosis; spindles originate in the cytoplasm. These nuclear features typical of plants, then, are fully developed only in the highest plants, and are the outcome of a long evolutionary process.

ANIMALS

The only known organisms not accounted for in the foregoing treatment of Monera, Protista, and Plantae, are those which the zoologists call Metazoa. To these, by the present treatment, the kingdom Animalia is limited. All are multicellular, holozoic in nutrition (with exceptions), and (again with exceptions) diploid as to all cells except the gametes. The bodies include freely wandering amoeboid cells. The sperms bear flagella. These characters indicate an amoebo-flagellate ancestry, like that of the Fungi and the various groups of Rhizopoda (as the amoebo-flagellate complex is not in itself a natural group, there is nothing to indicate that the animals, Fungi, and Rhizopoda are related through any ancestor more recent than flagellates).

Centrosomes are present, outside of the nuclei, in the cells of animals; at each mitosis they divide; the spindle is formed between the daughter centrosomes, and enters in among the chromosomes only as the nuclear membrane dissolves.

There are also embryological characters which bind together the great majority of the species. The developing individual passes through a stage in which it is a closed hollow sphere of a single layer of cells, a blastula. The blastula, by one series of stages or another, develops into a more or less spherical body whose wall is a double layer of cells pierced by an opening to the interior; this is a gastrula. An adult *Hydra* is a slightly modified gastrula; a man or a beetle is, in individual development and in evolution, a profoundly modified gastrula. So far as these characters extend, there is a pervading uniformity to animals, marking the group as obviously natural.

Doubt must be acknowledged as to the position of one group. Porifera (sponges) are the most primitive of the groups regularly included in Metazoa. This is the one phylum of organisms whose assignment to a kingdom is made here without confidence. The sponges are clearly descended from the amoebo-flagellate complex, and are in many ways intermediate between the amoebo-flagellate complex and typical animals. It is, however, not certain that they can be construed as exhibiting the embryological

characters of typical animals, and if not, it is possible that their evolutionary origin may have been independent of that of typical animals. If this possibility is the truth, the sponges should be placed among Protista, as in Haeckel's original account of that kingdom, but one tends to assume that they represent a stage in the evolution of typical animals, and are legitimate members of the animal kingdom.

CONCLUSIONS

The evidence and argument presented have been to the effect that organisms can be arranged, naturally and more conveniently than in the past, in four kingdoms, as follows:

1. Monera (Haeckel). Organisms without nuclei, the cells solitary or physiological independent. Groups included, bacteria and blue-green algae. Ancestral form, the original form of life; it is believed to be most nearly represented among living organisms by the nitrifying bacteria. Nomenclatorial type, *Bacillus subtilis*.

2. Protista, Haeckel. Organisms, largely unicellular, with nuclei; typically with permanent nuclear membranes, centrosomes, and intranuclear spindles, though all of these may be lost in evolution; lacking the combinations of characters to be listed as characteristic of plants and animals. Groups included, Flagellata (construed as excluding Volvocales), Rhizopoda, Sporozoa, Infusoria, diatoms, red algae, brown algae, and Fungi. Ancestral form, the first nucleate organism; this is presumably most nearly represented among living forms by the Chrysomonadida. Nomenclatorial type, *Amoeba Proteus*.

3. Plantae, Linnaeus. Organisms (with few and derivative exceptions) having plastids containing the four pigments chlorophyll A, chlorophyll B, carotin, and xanthophyll, and producing true starch and cellulose. The primitive members are motile, unicellular, and have nuclei much as in Protista; the higher are non-motile and of elaborate structure and have no centrosomes nor intranuclear spindles. Groups included, Chlorophyceae and Embryophyta. Ancestral group, Volvocales.

4. Animalia, Linnaeus. Organisms which are multicellular, typically diploid and holozoic, passing through blastula and gastrula stages in development. Centrosomes are present; spindles are generally formed outside the nuclear membrane, and enter the nucleus only as the membrane dissolves. Groups included, the Metazoa as usually construed (except possibly Porifera, which might fall into Protista). Ancestral group, Porifera; or, if that be excluded, Coelenterata.

ACKNOWLEDGEMENTS

Acknowledgements of more than one kind may be in order. The standard taxonomic revision is the work of an expert in the group concerned; it cites all pertinent literature; it is received with respectful interest (never with complete acquiescence) by the author's fellow experts in the same group, and is more or less annoying to others who have to take it into account, as requiring revision of familiar ideas of the limits of groups and the application of names. The present paper, on the contrary, issues from no expert; there lives now no Aristotle or Linnaeus, no one who is an expert on the whole range of life. Accordingly, it may perhaps be an annoyance to all who take cognizance of it. But one who thinks as a taxonomist is unable to withhold his hand from what appears to be an opportunity to make the taxonomic system more natural.

Associated with the fact that this is not the work of an expert is the fact that the bibliography is fragmentary. The literature pertinent to the present subject is a major fraction of the whole literature of life. I have studied what literature was readily at hand; I have leaned heavily on the advanced textbooks (5, 9, 11, 18, 36, 57, 81, 84, 85, 90, 97, 103, 105, 106, 123, 125); the original contributions consulted have been largely recent and largely American. I regret having overlooked many pertinent contributions, which it would have been only just to have

cited, but I believe that the evidence assembled is typical of the whole body and makes a strong case.

No part of the data presented is my own discovery. Of the ideas, I can claim as my own only the delimitation of the kingdom Protista. The remaining ideas are assembled from many sources—from reading, from conversation, from instruction. My teacher, in principles of classification, has been my father, Dr. E. B. Copeland; in cytology, Dr. C. E. Allen; in the science of algae, Dr. G. M. Smith. I am keenly aware of my indebtedness to them, and again to my father, and to my colleagues, Dr. H. J. Child and Dr. H. C. Day, for interest during the preparation of this paper and for searching criticism of fact, of inference, and of presentation. But no responsibility for the outcome is to be attributed to any of these gentlemen.

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BIOLOGICAL EFFECTS OF POPULATION DENSITY IN LOWER ORGANISMS

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INTRODUCTION

PEARL (1937) defines a population as "a group of living individuals set in a frame that is limited and defined in respect of both time and space." The science of group biology, he points out, "aims to describe the attributes and behavior of a group as such, that is as an entity in itself, and not as the simple sum of the separate attributes of the single individual organisms that together make up the group." The three principal group characteristics are size, growth, and quality. The latter refers to the genetic constitution of the population and its phenotypical representation. It is obvious that the genetic constitution is an important factor influencing the reproduction rate which in turn determines the primary group characteristic of "size" at any particular instant of time. Chapman (1928) has attempted to make a mathematical generalization of the interaction of these three factors in any population, borrowing his terms from Ohm's law of electrical conduction which is usually expressed as $\left(\frac{E}{B} = I\right.$ where $E =$ the potential, $R =$ the resistance, and $I =$ the current). He calls the major component of importance for population physiology in Pearl's qualitative factor the "biotic potential," corresponding to E of Ohm's law, defined as the "mean maximum rate of reproduction in given period under given conditions."

Every species of plant and animal has its own inherent "biotic potential" as every electric battery has a particular electric potential independent of the environment. The death rate, shortening of life span and fertile period, and the lowering of fertility and fecundity, etc. is called the "resistance" (R) which the environment puts up to the "biotic potential." The interaction of these two factors determines the value of I, the net rate of growth of the population. I in time, of course, determines the population density which, as will be shown later, is an extremely important component of R, the environmental resistance. Therefore, I is the instantaneous rate of growth determined by its own previous values and the time through which they have been acting. It should be noted here that this "Ohm's law for biology" must not be examined too closely, and though the idea is admirable Chapman is probably a little too optimistic about its applied value.

In 1838 Verhulst (1838) discovered that the S-shaped curve, which he called the "logistic curve," was a good fit for the growth of human populations. The first derivative of this curve gives the rate of growth in respect to time or in respect to size of population. This curve was later independently rediscovered as a population curve by Pearl and Reed (1920) who found it to be an excellent description of the growth of a great variety of animal

and plant populations, as well as of human populations; in short, that it expressed a general law of population growth, probably for fundamental biological reasons. Reed and Berkson (1929) have described the characteristics of the logistic curve for various values of the constants and explained the most satisfactory methods of fitting it. Pearl (1925, 1927, etc.) has shown that the logistic as well as expressing population growth in man, *Drosophila*, bacteria, yeast, etc., also may describe the growth of an individual (rat, tadpole, pumpkin, etc.).

An analysis of the logistic curve itself throws considerable light on the group attributes of populations at various stages of growth. Miner (1932), for example, showed that the birth rate in a logistic population can be expressed as a function of the population density and that this functional relationship is the same as that between the mean free path of the molecules of a gas and its density. This conclusion would seem at least to hint that the effect of crowding on the birth rate is a direct effect of interference between the individuals rather than an indirect nutritional effect as was generally believed. It will be shown later that experimental work seems to substantiate this idea in a few particular cases. But Pearl and other workers soon became interested in determining the biological nature of the various factors lying back of the production of the growth curve and set up experiments to analyze them bit by bit. Recently, Pearl (1938) has done valuable work in determining fertility factors in man with special reference to age, coitus rate and use of contraception. But due to greater ease of handling and to financial limitations most of the more particular analysis has been accomplished by means of animal experimentation.

At first glance, it would seem to be a virtually hopeless task to make a study of population leading to any broad generalizations due to the great diversity of types of aggregations of living organisms. For example, many believe that the population physiology is fundamentally different in an aggregation of animals held together merely because of limitations of space or because of mutual attraction for the same environment (moths about a flame) and a "society" held together by "social instinct" apparently for the "purpose" of mutual benefit. Alverdes (1927) bases his classification of animal aggregations on this distinction. Yet, non-social animals, man, and individuals which may be considered as a highly organized population of cells all follow the same growth curve. Bodenheimer (1936) has recently shown that highly organized colonies of termites, ants, social wasps, bumble-bees, and social bees all grow logistically and states that "the growth of these colonies resembles that of organisms." Mainly because the social or semi-social animals are very difficult to work with experimentally, in almost all of the experimental work about to be described the population density has been controlled merely by the size of the environment (rather than by crowding conditioned by the instincts of the animals themselves). Therefore, highly developed social activity does not enter as a complicating factor although the behavior reaction of the animals toward one another at various densities undoubtedly does play a part. Because of the similarity of end result previously mentioned, there is reason to believe that much of what has been discovered by the study of such relatively simple types of animal grouping also applies to organized societies, though with certain modifications.

POPULATIONS OF MICROORGANISMS

1. Rhythms and cycles in fission rate

The effect of crowding on the growth rate of a population was probably first studied experimentally by Woodruff (1911) working with *Paramecium aurelia* and *P. caudatum*. He placed these animals in from 2 to 40 drops of hay infusion, changing them to new medium every day, or in some cases every other day, and followed the growth cultures for from 16 to 20 days. He found that for both species the greater the volume of culture medium used the more rapid was their rate of reproduction. Likewise, in the 2-drop cultures the division rate was greater when they were changed to new medium every day than when they were only changed every other day. When he used culture medium in which *Paramecia* had been growing for a number of days and then strained off the organisms, other *Paramecia* placed in it did not have as high a division rate as they had when placed in a fresh medium. From these facts he concluded that *Paramecia* excrete substances toxic to themselves and that these substances are more effective when the organisms are confined to a limited volume of culture medium. Therefore, he reasoned, excretory products play an important rôle in determining the period of maximum numbers, the growth rate, and the rate of decline of populations of these animals.

These results were so clear cut and so reasonable that they were not questioned for a number of years while interest centered about the question of the necessity of conjugation and encystment and their place in the life cycle. It was generally agreed at the time that populations of infusoria go through "life cycles" from the "birth" of a new culture with a period of growth followed by a period of decline, old age, and finally death unless the race is "rejuvenated" by conjugation, encystment, or a change of environment. Within the cycles Woodruff (1905) also reported minor fluctuations which he called "rhythms." He defined a rhythm as "a minor periodic rise and fall of the fission-rate, due to some unknown factor in cell metabolism, from which recovery is autonomous." Both "rhythms" and cycles were demonstrated in *Oxytricha fallax*, *Pleurotricha lanceolata*, and *Gastrostyla steinii*. Gregory (1909) reported that the ciliate *Tillina magna* "shows the normal rhythmic fluctuations observed by Woodruff" and other workers found the same to be true of various other protozoa. Pearl (1907) reported a particularly interesting biometrical study on the effects of conjugation on *Paramecium caudatum*. By measuring a large number of conjugating and non-conjugating pairs in the unfavorable environmental conditions which lead to conjugation, he found that *Paramecia* alike in respect to length tended to pair, and that conjugation appeared to restrict variation and lead to stability of type rather than the reverse. This is interesting in light of recent assertions, based on theoretical considerations, that the value of sexual reproduction and therefore its evolutionary development was due to its leading to a greater number of variations for natural selection to operate upon. Moody (1912) reported that *Spathidium*, an organism which depends upon *Colpidium* for its food supply encysts if its numbers increase to a point where it reduces the *Colpidium* population to the vanishing point.

In 1911 Woodruff and Baitsell discovered that they could breed *Paramecium caudatum* indefinitely without conjugation by the use of a good culture medium (beef extract) or by varying the culture medium, but

that the rhythms persisted. By 1914 Woodruff and Erdmann were willing to say that "the so-called life cycle is non-existent." They accounted for the rhythms as resulting from an internal phenomenon, which they called "endomixis," during which a complete new nucleus of micronuclear origin is formed. Erdmann and Woodruff (1916) showed endomixis for the three races of *P. aurelia*. Woodruff (1925) expresses the opinion that the rejuvenation common to both conjugation and endomixis is due to intercellular changes apart from genetic factors. He has now, Woodruff (1926), bred *P. aurelia* for 11,000 generations without conjugation but with endomixis and resultant rhythms in fission rate occurring at intervals.

It is by no means certain that endomixis is any more essential to the continuation of life than is conjugation. Patten (1921) bred for eight months or 652 generations a pedigreed line of *Didinium nasutum* all progeny of the same conjugant which had no micronucleus. Rhythmical periods of depression followed by increased vitality occurred resembling those generally associated with endomixis; but endomixis never occurred, no micronucleus ever appearing in these animals in any period. Those animals which encysted or conjugated invariably died, "a fact undoubtedly related to their amicronucleate condition." As far back as 1917 Mast reported that he was able to raise *D. nasutum* without finding any cycles in their death rate or reproductive rate and found no evidence that either encystment or conjugation is a rejuvenating process. Beers (1928) reports that he raised *D. nasutum* under "practically" constant conditions for 265 days (925.5 generations) without the appearance of endomixis or any rhythms in the fission rate such as Woodruff describes. However, he was able to induce rhythms easily by slight variations in the food and thinks that this may account for the phenomena observed by Woodruff. It may be, then, that Pearl's discovery that conjugation preserves the stability of the species is the key to the real value of the nuclear reorganizations rather than a "rejuvenating" process. If a "run down" culture tends to produce variations, a return to the old type of organization of proven survival value might well appear to be "rejuvenation" as in the great majority of cases the conservative type would be better than new random variations, once good environmental conditions were restored.

In any case, conjugation, encystment, and endomixis are extremely important factors in the normal population physiology of these organisms. Barker and Taylor (1931) have shown that encystment is ordinarily dependent upon a high population density in *Colpoda cucullus*. When these animals are not numerous the encystment is dependent "upon a cumulative alteration of the medium brought about by the animals themselves." The greater the population density the sooner encystment occurs while the pH of the medium and the food concentration have little effect. Though the other authors do not directly state it, their experiments with *Paramecium* and other infusoria seem to indicate that overcrowding is one of the most common factors which leads to unfavorable environmental conditions and consequent conjugation.

2. *Allelocatalysis*

There was no questioning of Woodruff's (1911) evidence that crowding was unqualifiedly detrimental to infusoria and little or no further interest was taken in the matter until 1921 when Robertson published a series of papers in which he sought to prove that just the opposite was

true. He first found (Robertson, 1921a) that while a temperature of 30°C. kills isolated individuals of the small infusorian *Enchelys parcimen*, a culture initially containing a large number of them will survive and continue to multiply even at considerably higher temperatures. In other words, the group has a protective value for its individual members. Then he found that if he let a hay infusion become bacterized by standing for from 24 to 72 hours, the infusoria placed in it had a much higher rate of division than in a newly made infusion and that the period of declining fission rate was put off considerably. From this, he postulated that the bacteria liberate an "X-substance" autocatalytic for reproduction. Shortly after this, Robertson (1921b) found that if he isolated two individual infusoria in one drop of culture medium the division rate during the first 24 hours was higher than it was if he isolated just one individual per drop. From this he reasoned that the infusoria themselves liberated a substance identical to the "X-substance" he claims was produced by the bacteria. The effect was not due to the infusoria inoculating the culture with bacteria since it occurred in bacterized medium as well as in a medium filtered free of bacteria.

In a series of papers which followed, Robertson (1922, 1924a, 1924b, 1924c, 1927) expanded and refined his explanation of this phenomenon which he called the "allelocatalytic effect" or "allelocatalysis." On the factual side, he stated that "an old culture-fluid contains no substances which are toxic for infusoria, nor does it retard the multiplication of infusoria isolated into it from young cultures"; old culture medium, on the other hand, contains a substance "which enhances the multiplication rate of isolated infusoria"; the smaller is the volume in which a single infusorium is isolated, the greater is the initial reproduction rate of the culture, etc. He found that the reproduction rate may be sixteen times as great when two animals are isolated in a single drop of medium as when one animal is so isolated. By way of explanation, Robertson (1922) said: "The accelerative agent in cellular multiplication originates in the nucleus, and the autocatalytic time-relations which distinguish all types of growth are referable to the fact that nuclear synthesis is autocatalyzed." The "X-substance" is a portion of this nuclear catalyst liberated into the surrounding medium at the time of cell division.

These papers immediately attracted a great amount of interest on the part of a considerable number of workers. For some time no one was able to reproduce Robertson's results and the fact of "allelocatalysis" was disputed, not to say the explanation.

Cutler and Crump (1923a and 1923b) were unable to find any evidence of allelocatalysis in *Colpidium colpoda* and concluded that it did not exist for this animal at least. Robertson (1924b) replied that he could obtain it with *Colpidium* using Cutler and Crump's medium and claimed that they did not find it due to faulty technique in not washing their animals and so transferring some of the old medium with its "X-substance" to the new medium. Cutler and Crump (1924) added to their previous work showing that the number of divisions of *Colpidium* in a given time was "intimately associated with the size of the bacterial population" and that the number of divisions steadily decreases as the number of animals inoculated in a given volume increases.

Greenleaf (1924 and 1926) conducted a long and well-controlled series of experiments on *Paramecium aurelia*, *P. caudatum*, *Pleurotricha lanceolata*, and *Stylonychia pus-*

tulata, rearing various numbers of animals in 2, 5, 20, and 40 drops of hay infusion both fresh and old. His results entirely backed up Woodruff's (1911) original assertion that the metabolic waste products are harmful to the animals. No allelocatalytic effect was observed in any series. The division rate of a single animal was greater than that of two animals originally placed in the same volume of fluid.

Peskett (1925) found no allelocatalytic effect in the growth of yeast. Myers (1927) met Robertson's objection to the work of Cutler and Crump by washing his animals before placing them in fresh medium, but still found no allelocatalytic effect. Darby (1930) working with yeast and *Paramecium* and Phelps (1935) working with the ciliate *Glaucoma pyriformis* in bacteria-free culture media likewise could find no such effect as reported by Robertson.

On the other hand, Yocom (1928) raising *Oxytricha* on a sterile medium of .05 of one per cent of beef extract found an allelocatalytic effect and thought that Robertson's explanation was more satisfactory than any other. In his experiments, he placed 4 drops of medium in one depression and 10 drops in another depression of a glass slide, isolated one animal in each depression, and counted the number of animals at the end of 24 hours. At 28°C. the 4-drop cultures had on the average 10.2 individuals as compared with 9.08 individuals in the 10-drop cultures. At 23°C. the number of individuals in 24 hours was 9.07 compared with 8.10 and 3.54 compared with 2.91 for the 4-drop and 10-drop cultures respectively.

Peterson (1929) working with *Paramecium caudatum* found no difference in reproduction rate in 2 and 4 drops of medium between 1 and 2 animal cultures whether the animals were washed or unwashed and found that one animal isolated in "conditioned" medium had a greater division rate in a large volume than in a small volume. But unwashed animals placed in sterile medium had a higher division rate in a small volume than in a larger volume. When 1, 2, 4, or 8 unwashed animals were placed in 1, 2, or 5 drops of bacterized medium, the greatest reproduction rate occurred in 5 drops of medium originally containing one animal. But when either washed or unwashed animals were placed 1, 2, or 4 at a time in 20 drops of bacterized medium the highest division rate occurred in the 4-animal cultures and the lowest occurred in the 1-animal cultures. He thus demonstrated an allelocatalytic effect under certain conditions but at the same time showed that it depended upon a more complex balance of volume, food supply, and number of animals than Robertson had imagined.

Luck, Sheets, and Thomas (1931) showed the great importance of the food supply, particularly bacteria, in the division rate of *Euplotes*. Loefer (1936) showed the importance of salts as well as organic materials in the growth of *Paramecium bursaria*. Smith (1932) working with *P. caudatum* beautifully demonstrated the importance of an optimal food concentration. He showed that a particular optimum food concentration gave a far higher reproductive rate than any greater or lesser concentration and came to the conclusion that at the beginning of a new culture this plays a greater rôle than the number of animals present. Johnson (1933) found the same to be true for *Oxytricha*, the food concentration being determined by the number of bacteria present. At certain concentrations of food he found that single animal cultures had higher reproductive rates if started in smaller volumes than if started in large

volumes and that in the same volume 2-animal cultures had a higher fission rate than 1-animal cultures eq. (allelocatalytic effect). Johnson (1936) elaborated on these experiments, this time using *Paramecium caudatum* and varying both the bacterial concentration and the number of animals at the start and observed the cultures for a seven-day period. A certain "x-concentration" of bacteria gave the highest rate of reproduction in both 1- and 5-animal cultures on the first day; while after the first day, 5-animal cultures and after the second day, 1-animal cultures had a higher division rate in 5 x concentration than in x concentration of bacteria. After the first day, 1-animal cultures had a higher division rate than 5-animal cultures in x concentration of bacteria. During the second day 5-animal cultures had higher division rates than 1-animal cultures in 5 x concentrations but after the third day the 1-animal cultures had the higher division rate.

These latter experiments all indicated that the reproductive rate of infusoria is in some way controlled by the interaction of numbers, concentration of food, and volume of the medium rather than by any of them alone. The experiments of McPherson, Smith, and Banta (1932) throw a great deal of light on this. They found that when a strong culture medium (one with a high food concentration) was used the allelocatalytic effect was observed but when they used a weak culture medium the reverse occurred. These results were obtained both with *P. caudatum* and with parthenogenetically reproducing females of the water flea *Moina macrocopa*. This suggests as the most likely explanation of all the phenomena observed that the effect of numbers on reproduction rate depends upon whether the presence of the animals themselves is shifting the environmental conditions nearer to or farther from the optimal point. Thus, if there is too great a concentration of food a larger number of animals might be expected to lower the concentration and thus create better conditions for themselves. On the other hand, if there is not enough food, the reduction of the available supply by the animals would make matters still worse for them, and so lower the reproductive rate. Such an explanation seems more rational that bringing in an "X-substance" which no one has been able to isolate or otherwise directly demonstrate. However, the protective value of the group against high temperatures is yet to be explained. Jahn (1934) and Johnson (1937) have reviewed the literature and theories dealing with this phenomenon. The latter agrees in general with the type of explanation just given and the former points out that besides changing the food supply the animals themselves might bring the pH, the oxidation-reduction potential, and the CO_2 concentration nearer to the optimal point.

AQUATIC ANIMALS

At about the same time, Drzewina and Bohn in France and Allee and his associates in America found certain remarkable effects of crowding on aquatic animals and started long series of investigations to clear up the matter. Allee (1931 and 1934) has fully described the results of this work, so as it is of less immediate interest to the subject of this paper than literature to be described later, I will only outline it here.

The work of Drzewina and Bohn (1920–1927) and Bohn and Drzewina (1920–1932) all centers about the effect of toxic substances in water in which animals are living. *Convoluta*, a small turbellarian, dies quite quickly if put in fresh water. It was found, however, that a group of these animals can survive when a propor-

tion of fresh water is added to sea water which kills a single animal in the same volume (Bohn and Drzewina, 1920). In other words, the group has a protective effect against this unfavorable environmental condition. Colloidal silver added in small proportions to sea water proved to be very toxic to *Convoluta*, but a group of these animals was able to survive in concentrations which killed single individuals (Drzewina and Bohn, 1921a), which phenomenon was named "autoprotection" by the authors. Groups of tadpoles also protected themselves from the toxic effect of colloidal silver which killed single individuals. Two small tadpoles placed in a small volume of a colloidal silver suspension will live indefinitely, while if placed in a larger volume with the same concentration of silver they soon die (Drzewina and Bohn, 1921b, c, d).

The spermatozoa and eggs of the sea urchin respond in the same way to crowding under unfavorable conditions. Spermatozoa were treated with dilute suspensions of neutral red (Bohn and Drzewina, 1923) and colloidal silver (Drzewina and Bohn, 1920) and with dilute solutions of potassium chloride (Drzewina and Bohn, 1923) and in each case the larger the number of spermatozoa the longer they retained their power to fertilize eggs. Bohn-Drzewina (1926) likewise found that a large group of spermatozoa could withstand high temperatures which are fatal under less crowded conditions. It has always been something of a puzzle why such enormous quantities of spermatozoa are produced in most species when so very few are used. But if a single spermatozoon dies under conditions which a mass of them survive, then the chance for successful fertilization of eggs when large numbers of spermatozoa are produced is far greater than the product of an isolated individual's chance for success times the number of individuals. Thus the great majority of spermatozoa that are doomed to die first perform the useful service of protecting their more successful rivals.

The effect on sea urchin spermatozoa of a concentrated solution of CO_2 is more puzzling (Drzewina and Bohn, 1926c). Spermatazoa in large numbers exposed to a concentrated CO_2 solution for a few moments do not lose their mobility and are able to fertilize eggs, but no fertilization membrane forms and the eggs develop abnormally. Spermatazoa in smaller numbers under the same conditions lose their mobility but recover in about a half an hour and can then fertilize eggs which will develop a fertilization membrane and then develop normally. When left in this solution for from 20 minutes to one hour, a large mass of spermatozoa gradually recovers the ability to fertilize eggs with normal results while a small mass loses more and more the ability to do so.

Colpidium, *Paramecium*, and other infusoria, *Hydra* and several planarians were all found to be protected to a greater or less degree by crowding from the poisonous effects of colloidal silver, various salts, and other substances (Drzewina and Bohn, 1921c, e). One might suppose that the power of the group to protect would be in inverse proportion to the concentration of the poisonous substance (if the protection is due to the animals giving off some antitoxin) but these authors find that the survivorship is dependent to a very much greater degree upon the density of the animal population than upon the concentration of the poison (Drzewina and Bohn, 1921e, and 1923). But crowding does not always have a desirable result for these forms and in fact just the opposite effect; auto-destruction occurs under some conditions (Drzewina

and Bohn, 1922). Potassium chloried added in small quantities to sea water proved to be toxic and tests were made in which (a) 25 *Convoluta* were placed in 2 cc., and (b) 250 *Convoluta* were placed in 2 cc. of such a solution; the concentration of KCl being the same in both cases. In the lower density (a), most of the animals died in one to two minutes but some of them lived. But in the high density, (b), some sticky substance was produced by the animals themselves and in a very short time they were all stuck together and soon died. The authors compare this to the agglutination of bacteria produced by antibody in the blood of a host. Likewise, the small fresh water planarian *Polycelis nigra* was able to survive in a large volume of a dilute KCl solution but died in a small volume of the same solution (Drzewina and Bohn, 1921e). The authors offer the explanation that the worm responds to the KCl by putting out some substance, presumably to counteract its effect, but that this substance is poisonous to them (under these conditions at least) and in small volumes becomes sufficiently concentrated to kill them.

It was also found that the presence of certain insoluble solids would have an effect on animals in liquid medium (Drzewina and Bohn, 1926a, b, d; 1927, a and b; Bohn and Drzewina, 1932). *Convoluta* die quickly if placed in a silver container in the light but survive several hours if in the dark. The same is true if the walls of the glass container are coated with stearine, but tin and paraffin are as harmless as glass. As water which has stood in a silver or stearine container is not toxic, the effect is not due to a soluble poison. The authors suggest that the silver acts as a catalyst for some harmful reaction. Strangely enough, if *Convoluta* are put in a container with both silver and tin sheets, the latter protects them from the former and they live unharmed! Sea urchin sperm lose their power to fertilize eggs very quickly if metallic silver is present, but a large group of them exhibit mass protection and can retain their power to fertilize for over an hour in a silver vessel. Metallic silver does not always have a bad effect however; while it inhibits the growth of the roots of tobacco seedlings, it accelerates the growth of the roots of water cress seedlings (Bohn and Drzewina, 1932).

Allee and Bowen (1932) tested the effect of colloidal silver on gold fish and found that a group of them survive longer than a single individual placed in the same volume and concentration. On the other hand, if various numbers of fish are used but the volume of water and concentration of silver are kept the same *per fish*, the group has no superior protective value over the single individual. In other words, the degree of protection is determined by the volume of gold fish as compared with the amount of silver and if this proportion remains constant the number of individuals does not matter. Carpenter (1930) determined how great a concentration of salts of heavy metals (Copper, zinc, etc.) could be withstood by fresh water fish. Salmonidae are particularly sensitive to extremely dilute metallic ion solutions, such as water which has come in contact with mine workings or passed through metal pipes, and this is a serious practical problem. She worked out the following relationship for the survival time of fish placed in such toxic reagents:

$$K = \frac{1}{t} \log \frac{1}{\text{molar concentration}},$$

where t = survival time and K = a constant for the species of fish. In such solutions, the fish rapidly excrete mucus which tends to fix the metal salts and remove them from solution. Thus in a

limited volume a group of fish is able to protect its component individuals from the poison, the mucus acting as an antitoxin, and the greater the bulk of fish (both as to number and as to size) the greater will be the protection. If the original fresh water fish are removed from a tank of water which contained metal salts and other fish are put in, these newcomers will survive much longer since the first fish removed some of the salts and so *conditioned* the water for them. There are harmful effects of overcrowding however, and in pure water a large group of fish will not live as long as a smaller group. These harmful effects of crowding are so apparent that they have attracted little interest, most workers preferring to study the beneficial effects. Shaw (1932) showed that the presence of mussels has a beneficial effect on the growth of fishes placed in water with sub-lethal doses of $HgCl_2$ and NH_4Cl.

While fresh water animals may remove metallic salts from water and so protect themselves, Allee (1928) has shown that salt water animals may increase the salinity of fresh water in which they are placed. Thus a number of turbellarian worms, *Procerodes* a marine form, may survive in a limited volume of fresh water while a single worm dies. Allee (1933) showed that water from old *Procerodes* cultures protect these animals when they are placed in fresh water.

Stone head catfish, *Schilbeodes melas*, occasionally aggregate under natural conditions. Working with them in the laboratory, Eddy (1926) says that "the rate of carbon dioxide production showed that the rate of respiration was decidedly increased by even the presence of one other individual." The black catfish, *Ameiurus melas*, normally forms bunches and Eddy found that fish just removed from the center of a dense, active aggregation have a higher rate of oxygen consumption than those from small aggregations of two to four individuals while fish which voluntarily isolated themselves from the group had a considerably slower rate of oxygen consumption than either. Fish taken from the center of a dense, active, naturally formed group and isolated for several days still had a higher rate of oxygen consumption than individuals that voluntarily isolated themselves. This latter fact might be interpreted either as meaning that grouping has a lasting effect on metabolism or that the rate of metabolism influences the tendency to form groups.

Allee (1927) says that brittle starfish, *Ophioderma brevispina*, normally aggregate in some shaded spot or about some quiet individual. This tendency seems to be due to mutual attraction to the same environmental conditions rather than to social instinct. The members of such groups usually live longer than isolated individuals, but under certain bad environmental conditions the reverse is true. Crowding was found to lessen at first the oxygen consumption of the individuals but after a period of about six hours to increase it. Allee and Fowler (1932) later found that this response varied seasonally. The first oxygen consumption test had been made in the autumn, outside of the breeding season. A test made during the breeding season under the same conditions showed that when first crowded the starfish consume more oxygen than isolated individuals at this time of the year. Complicating factors rather difficult to understand were discovered when in further tests glass rods were placed in the containers to simulate the eel grass in the natural environment. With these rods present the starfish responded to crowding in the breeding season as they had responded (in

respect to oxygen consumption) outside of the breeding season without glass rods! Much more work will be required to clear up this puzzling matter.

Allee (1925 and 1926) found that land isopods have a strong tendency to bunch under some conditions (a dry substrate being the most common cause) and this bunching lowers the oxygen consumption and carbon dioxide output (per unit weight of isopods) for the first hour or two and then increases it. The fact that the animals remain quiet when first grouped is a partial explanation. Land isopods require a very damp environment and die if there is not enough moisture. As grouping lessens the rate of evaporation, it has a protective value in dry periods which is the time when the groups naturally tend to form.

Adolph (1931) found that crowding tadpoles has little effect on their early growth but that crowded animals stop growing much sooner than isolated individuals. He demonstrated very convincingly that this is not due to any conditioning or poisoning of the water but simply due to the mechanical (or psychological) interference of the animals with each other. The crowding effect was just as marked when fresh water was continually and rapidly run through the container as when it was not changed; while merely partitioning the individuals off from one another without changing the volume of the container eliminated the bad effect of crowding. Simply agitating the individual tadpoles had the same inhibiting effect on growth as crowding. Shaw (1932) on the other hand "conditioned" water by letting animals live in it for a while and found that *Amblystoma* larvae regenerated amputated tails quicker in such water than in fresh water. Filtering conditioned water destroyed this growth stimulating quality. Allee, Bowen, Welty, and Oesting (1934) found that adult fish grow less under crowded conditions than when not crowded, particularly if the water is not changed. But there was some indication of an optimal density of more than two fish per aquaria for the growth of young fish in the first few days and Church (1927) reported the same thing. In general, they found that young fish grow more rapidly in homotypically conditioned water than in fresh water. Straining out the faeces, autoclaving and evaporating and rediluting the conditioned water did not change its effect. These fish lived considerably longer in conditioned distilled water than in freshly distilled water since they lose electrolytes rapidly to the latter. So the beneficial effect of conditioned water, in a few cases where it is found to be beneficial, is to be accounted for partly at least on the basis of a more nearly optimal osmotic pressure. Greenberg and Schmidt (1936), Wulzen (1929) and many others have found that certain food substances such as egg and certain extracts from animal tissues are greatly stimulating to the growth of worms and other aquatic animals. Allee, Oesting, and Hoskins (1936) therefore investigated the possibility that homotypically conditioned water contains food substances which would account for its growth stimulating quality. They found that regurgitated food and the faeces found in gold fish conditioned water were available as food for fish but they nevertheless felt that some other biochemical substance with growth promoting power may be present in conditioned water. Evans (1936), suspecting that conditioned water may be richer in vitamins, tested the growth of gold fish under the following conditions: (a) vitamin-rich diet, homotypically conditioned water, (b) vitamin-rich diet, fresh water, (c) vitamin-free diet, homotypically con-

ditioned water, (d) vitamin-free diet, fresh water. The animals grow best in conditioned water (a) and (c). In conditioned water the presence (a) or absence (c) of vitamins made no difference nor did it make a difference in fresh water (b) and (d). She concluded that vitamins are not the growth promoting factor in conditioned water.

As things now stand, the existence of any growth promoting factor, other than food, in homotypically conditioned water is yet to be definitely proven; and, of course, the nature of any such substance is quite unknown. This whole line of investigation is very similar to the search for Robertson's "X-substance" to explain allelocatalysis in the protozoa. Allee still seems to be quite open-minded on the question.

INSECTS

The previous experiments described were designed to explain a few particular aspects of the population problem in a variety of different animals. Two insect species, however, have proven to be such excellent experimental subjects for the purpose that it has been possible to analyze nearly every phase of their population physiology quantitatively as well as qualitatively. These are the common flour beetle, *Tribolium confusum* Duval, and the fruitfly, *Drosophila melanogaster*. Both are easy to raise under laboratory conditions and go through complete metamorphosis with sharply divided egg, larval, pupal, and imaginal periods. The flour beetle has the advantages that the laboratory culture conditions are almost identical to those of its natural habitat, it can be handled and the numbers counted at every stage of development, and the sexes can be distinguished in the pupal period; disadvantages are that it is relatively long lived with a considerable period from generation to generation, the sexes cannot be distinguished in the adult stage, and the handling is too laborious to permit the use of large numbers. On the other hand, tens of thousands of *Drosophila* can be used in an experiment; they have a relatively short life span, breed very rapidly, the eggs as well as the adults can be counted, and the sexes determined in the adult stage. The major disadvantage with *Drosophila* for population work is that no successful technique has been developed to handle, sex, and count large numbers at intervals during the larval period without running the risk of injuring them.

1. *Tribolium experiments*

Tribolium, according to Good (1933), apparently lived originally and can still be found under the bark of trees and in rotting logs. But almost any powdered food provides it with a good environment and today it is found throughout the world thriving in flour and other ground food. There are two common species, *T. ferrugineum* and *T. confusum*, very similar in appearance and in habits, the former, however, preferring a somewhat warmer climate than the latter. Both species can breed regularly all year around in heated houses, but otherwise they breed only in summer, passing the colder months in the adult stage. As practically all the experimental work has been done with *T. confusum* Duval, the following discussion will be confined to this species.

Park (1934c) has given a fairly detailed description of the general biology and living conditions of this animal. He customarily raises them in unbleached white flour at a constant temperature of 28°C. and at as constant a relative humidity as he can obtain. Under these conditions, the eggs hatch in from 5 to 7 days (average about 6 days), after being laid;

the larval period lasts from 30 to 40 days (average about 37 days); the pupal period from 6 to 8 days; and while the life span of the adults is not accurately known, more than half of them live for over a year and a fair number survive for over two years. The adult females produce eggs soon after emerging, but fertile eggs are seldom laid in less than two weeks time. This makes about nine weeks as the shortest average time from generation to generation, while for the practical problem of carrying through experiments only four to five generations can be conveniently run in a year's time. The beetles and their eggs can be separated from the flour by sifting and Park (1934c) has designed a mechanical sifter to speed up this process. Since the sex of the adults cannot be determined by inspection without killing the beetle, it is necessary to separate the sexes in the pupal period in order to set up a population of known imago composition.

Fortunately, disease has not interfered with the experimental work so far, although White (1923) and Riley and Krogh (1922) have reported two different infectious diseases of *Tribolium*.

The value of *Tribolium* as an experimental animal was demonstrated by Chapman's (1924) study of the effect of nutrition on growth and development. He found that food is not so important a factor for survival and growth as for metamorphosis, for which the proper nutritional requirements have to be met. He decided upon whole wheat flour as the best culture medium and incidentally found that frequent handling greatly increased the death rate. As a result of this experiment, Chapman has used whole wheat flour as the culture medium in all the work he has done since.

Chapman (1928) made up a series of cultures each in an individual bottle with 32 grams of whole wheat flour and alike in all respects except that they were started with initial populations of 2, 4, 8, 16, 32, and 64 beetles per bottle. He then took a census of each of these cultures at intervals for a period of 139 days and found the interesting result that at the last count (made on the 139th day) the six cultures had pretty close to the same number of beetles, the average being 43.97 ± 2.88 individuals per gram of flour. A second experiment reported in the same paper consisted of placing one pair of newly emerged beetles in 16, 32, 64, and 128 grams of whole wheat flour and following the population growth in each. Gause (1931) analyzed these data and found that in each case the growth was logistic and reached an upper asymptotic value. The greater the volume of flour, the greater was the upper asymptote of population, a simple exponential relationship existing between those two variables. This was taken to mean that the maximum size obtainable by a growing population in a limited environment is a direct function of the size of the environment. Since the populations started with different numbers of individuals but reached approximately the same maximum density, it necessarily followed that density determined the net rate of increase at some point. Allee (1931, p. 180) plotted from Chapman's data (on the first of the two population experiments just described) the net rate of increase against the initial population per 32 grams of flour. The striking feature he calls attention to is the fact that the cultures started at the lowest population density had the lowest rate of reproduction in the first few days, that there was an optimal initial density (4 beetles per 32 grams of flour), and that greater densities than this successively lowered the growth rate for the first 25 days at least. This, as he points out, is similar in appearance to Robertson's

phenomenon of allelocatalysis in the protozoa. Chapman's experiments were run on such a small scale that the results were by no means conclusive, so Park (1932) repeated the experiment, counting the eggs produced as well as the larval, pupal, and imaginal population and obtained the same general result. He found that 2 pairs of beetles in 32 grams of white wheat flour produce more eggs per female day in the first few days than does one pair of beetles in the same volume of medium but that crowding above this point results in a progressively lower fecundity. In all cases, as the population grows the fecundity declines until a state of equilibrium is reached where the population remains fluctuating about its asymptotic value.

Park was wise enough not to follow Robertson's lead and postulate some mysterious "X-substance" to explain this optimal density phenomenon. Instead, by combining the results of two cleverly conceived lines of investigation, he was able to arrive at a satisfactory first approximation to complete solution with a minimum of assumption. First, it was found (Park, 1933) that while virgin females will lay sterile eggs, copulation greatly stimulates fecundity and recopulation increases it still further and also increases the percentage of the eggs that are fertile. Secondly, he found that adult beetles eat their own eggs; males, virgin females, and fecundated females all eating them at approximately the same rate, while this practice has no effect upon the fecundity of the females. From these facts he argued (Park, 1933) that up to a certain point increasing the population density would increase the fecundity by increasing the frequency of copulation, while increasing the population density would reduce the net production of eggs by increasing the number eaten. The resultant of these two factors which work in opposite directions might be expected to produce the observed results, an optimal density of population for maximum net egg production per female beetle.

Having developed a reasonable theory to explain the beneficial effects of crowding up to the optimal point, it was natural to turn next to a further investigation of the factors underlying the depressing effect of higher population densities. Chapman (1926) showed that when *Tribolium* are rubbed or otherwise irritated they give off a gas which smells like an aldehyde, irritates the eyes and nose, and turns flour pink. An old culture which has been kept in a closed incubator for a long time always has the characteristic odor of this gas. Larvae exposed to a high concentration of the gas become deformed in about 10 per cent of the cases, the deformity usually taking the form of the larvae developing wings in the molt ordinarily producing pupae and then never developing further. Pupae so exposed sometimes develop into deformed adults. No such deformities have ever been observed in ordinary cultures, but it is quite probable that in old, crowded cultures the gas is present in sufficient quantity to have deleterious effects of a somewhat less drastic nature. Just why it should be formed is not known, though the idea that it may be used as a defensive weapon is the most obvious guess. Park (1934a, 1934b) started to attack the problem in a long series of papers, which are still being added to, by determining the effect of "homotypically conditioned flour" upon *Tribolium*. These experiments closely parallel Allee's work on the conditioned environment for other organisms. Flour is said to be "conditioned" when a large culture of *Tribolium* has lived in it for a long period of time. Mated female beetles placed in it laid significantly fewer

eggs than they did in fresh flour, other environmental factors being the same, and the egg production declined as long as they were left in it. A part of this additional decline with time is accounted for by the fact that fecundated beetles lay fewer eggs as they grow older, even in fresh flour. The beetles which had had their fecundity reduced by conditioned flour recovered their normal rate of egg production when returned to fresh flour if they were not left in the former too long. The conditioned flour apparently did not affect their fertility. Park (1935a) found that origin females lay three times as many eggs in fresh flour as in conditioned flour. On the other hand, the cannibalistic tendency is less in conditioned flour. Male beetles were used to test this point (as they eat eggs at the same rate as females and do not complicate matters by laying eggs), and it was found that they eat only half as many in conditioned flour as they do in fresh flour, other factors being equal. Still another effect of conditioned flour was to increase the time and variability in the time of larval metamorphosis. To determine how early in the life of a new culture the autoconditioning of the flour would produce an effect on the beetles, Park (1936b) set up five cultures alike except that the medium was fresh flour mixed with the proportion of 0, 25, 50, and 75 per cent conditioned flour. He found that the 25 percent conditioned flour culture produced significantly fewer eggs than the entirely fresh flour culture, there was no noticeable difference between the 25 and the 50 percent cultures but the 75 percent culture produced still fewer eggs and the 100 percent conditioned flour culture produced fewer eggs than any of the others. He concluded that the effect of conditioning on fecundity sets in "before the culture has reached a decadent state" but that it has no effect on fertility at any stage. Park and Woollcott (1937) repeated and added to this work, the principal new finding being that conditioned flour increases the mortality of larvae. Strangely enough, Park (1936) could find no significant effect of conditioned flour on either the oxygen consumption or on the weight of the adult beetles. An interesting point was to determine whether starting beetles in conditioned or partly conditioned flour would in effect increase the biological age of the cultures, as should be the case if conditioning is the major bad effect of overcrowding. Park (1938) set up 200 bottles each originally with 32 grams of flour and 2 pairs of *Tribolium;* 116 of them had all fresh flour, 36 had 5 to 10 percent conditioned flour, 36 had 15 to 25 percent conditioned flour and the remaining 12 bottles had 100 percent conditioned flour. A year later he examined all the cultures for dead as well as living beetles. There were no living beetles at all in the cultures made up with 100 percent conditioned flour and in the other cultures the total populations were in inverse proportion to the original amount of conditioned flour. It might therefore be concluded that the effect of conditioned flour was to "age" the culture and bring on decline and extinction earlier. Park (1937 and 1934b) and Ford (1937) have written general discussions of all this work up to those dates.

A fact of perhaps very considerable importance in a declining population was discovered by Holdaway (1933) who reported that he could vary the sex ratio by starving the larvae. Under ordinary conditions 51 to 52 percent of the pupae, and presumably of the adults also, are males. Holdaway obtained larvae within 12 hours after they hatched from the eggs, placed them in a moist chamber without food for one, two, or three days and then transferred them to fresh flour.

The larvae which were starved one day gave rise to 54 to 58 percent male pupae, somewhat, but not significantly, more than the ordinary proportion. But larvae starved two or three days gave rise to only 45 to 50 percent male pupae which was significantly less than in the controls. The highest observed mortality in any of Holdaway's series was 6.9 percent from the larval to the pupal period and this is not great enough to account entirely for the observed difference in sex ratio. Possibly a selective mortality combined with chance does account for it; otherwise, the only apparent alternative explanation is that starvation produced an actual sex change in some individuals. It would be interesting to know if the sex ratio of larvae developing in old densely populated cultures is likewise changed, but this has not yet been determined.

Park (1938) has recently presented some data on the effect of crowding *Tribolium* during the larval period. He reports that larval and pupal mortality and the duration of the larval period are increased and the weights of pupae and imagoes developing from them are less when larvae are subjected to crowding. However, if the culture medium of a crowded larvae population is changed every 48 hours none of these effects are seen.

Chapman is a strong advocate of applying quantitative as well as qualitative methods to biological investigations, an idea first forcefully introduced into this country for experimental work by Pearl in his studies on poultry and later on *Drosophila* as will be described further on in this paper. All population work is essentially quantitative in nature and the answers to many of its problems depend upon a comparative evaluation of the effect of qualitatively different variables. It would be desirable then, to have a single yard-stick on which to measure, let us say, all environmental factors. In this connection Crew (1937) has recently suggested that the sex ratio might be used as an index of socio-economic welfare in human populations. Chapman introduced his term, "biotic potential," particularly for use as just such a yard-stick. He says, (Chapman, 1928):

"It is suggested that environmental factors be measured in terms of their effect in reducing the potential rate of increase. Thus all factors may be measured on the same scale and their values compared directly."

This would be all very well if we were interested in nothing except the growth rate over a limited period, which is certainly a matter of prime importance to population physiology, or if we could assume that this is such a key phenomenon that all other factors could directly be determined from it. As we shall see later with *Drosophila*, one environmental factor, such as low temperature, may lower the net rate of increase and slow down the rate of development while at the same time cause the animals to grow larger and live longer, while another factor, such as overcrowding, may also lower the net rate of increase and slow down the rate of development but produce smaller animals which do not live as long. In this case, the net rate of increase would be a very poor scale indeed on which to compare the general biological effects of the two different environmental factors.

On the other hand, the mathematical formulae based on what Chapman calls the "biotic constants" may give a clearer idea of the relationship existing between different population variables and as predicting formulae may be of use in testing out various hypotheses. Chapman (1933) and Chapman and Whang (1934), for example, have worked with "synthetic populations" of adult male beetles only

to which they added eggs at the same rate at which it was calculated they would have been laid had half the beetles been females. In the latter paper an experiment is reported in which four bottles each with 32 grams of flour were started, three of them with 16 male beetles and one control bottle with 8 male and 8 female beetles. Eggs were added to all the male bottles at the calculated rate of oviposition for 8 females at this density and temperature. On the 33rd and 34th days, when 1,455 eggs had been added to each "synthetic population," 142, 150, and 141 eggs were found remaining in these three cultures while 135 were found in the normal control culture to which none had been added. The closeness of the results would indicate that the calculations on rate of egg eating, and rate of hatching of eggs, etc., the "constants" on which the number of eggs added was based, were not far from being correct.

The degree of accuracy with which predictions can be made naturally depends upon how closely the same results can be duplicated in successive experiments. Pearl's *Drosophila* experiments were customarily carried out with very large numbers, usually thousands, of flies which were also of quite definite and known genetical make-up. Using the same stock of flies, it is practically certain that the same relationships would obtain again if the experiments were repeated. That is to say, the growth curves would still be logistic and the same functional relationship would exist between fecundity and density of population, etc. Chapman, Baird, and Lillian (1934) criticize the use of *Drosophila* as an experimental animal on the basis that its biotic constants are too unstable to permit the use of them in mathematical formulae and point out the superior advantages of *Tribolium* in this respect. They cite a paper from this laboratory by Alpatov (1932) to show how unstable *Drosophila* is especially in respect to fecundity.

Philosophically Chapman's point seems a feeble one. *Drosophila* would appear to be quite as important an organism in the scheme of cosmic organization as *Tribolium*. Certainly it is quite as desirable and significant to know about the biology of one as about that of the other. Furthermore there appears no reason to manipulate any "constants" that may be found for any other purpose than to obtain hints for future experimental investigation. *Drosophila* populations do in fact show certain biological relationships which are constant on experimental repetition: Pearl and his associates have never attached any special significance to the arithmetic values of particular points picked from their fitted curves, which values might be called "constants" by Chapman's definition. They were not interested in selling insurance policies to flies or beetles either, but in showing sequences of events which when general and repeatable can be considered as biological principles, and this they have done. Certainly there are advantages in working with an animal which gives close to the same reaction every time it is treated in the same way. But it has yet to be demonstrated that *Tribolium* is measurably superior to *Drosophila* in this respect. Chapman, Baird, and Lillian (1934) report that in an experiment in which they placed 100 *Tribolium* eggs in each of two bottles at 32°C., 27°C., 22°C. and 17°C. the larval and pupal periods were about the same in each two bottles at the same temperature. As will be shown in another paper, the time for development is also very constant for *Drosophila* at low densities but has a large standard deviation in crowded bottles, and Park has shown that in conditioned flour the

variability for this factor is greatly increased in *Tribolium*, also. The former authors give data for the fecundity of 15 mated females for just the first 13 days out of a long life span and there appears to be a considerable variation from day to day for the same female beetle, and some of them laid twice as many eggs as others during this period. This, and the data of Park on *Tribolium* fecundity, do not appear to indicate that *Tribolium* are especially constant for this factor. But the stability of mean values depends quite as much on the numbers involved as on the standard deviation, and in this respect there is an enormous advantage in favor of *Drosophila*, even if it is a bit more variable, which is doubtful.

(*To be concluded*)

ON HUMAN SOCIAL BIOLOGY

I. PRELIMINARY REMARKS

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The study of the relations between man and other men can and has been undertaken in a variety of ways. Sociology, ethnology, psychology, psychiatry, and epidemiology deal with certain aspects of such relationships, some in a more general and theoretical fashion, others in a more particularized and specific manner. Notwithstanding differences in methodology, all of these branches of science have the same aim: to increase the knowledge about a particular kind of activity of *Homo sapiens;* and, as a consequence, to achieve the understanding of means by which man's sojourn on this planet may be made more agreeable and pleasant. This is also a fundamental objective of human biology, one of the most recent additions to those branches of science concerned directly and primarily with man. In the words of Pearl: "Human biology has for its purpose and ultimate objective the *synthesis* of the knowledge of the *biology* of man that has been acquired in the course of the development and evolution of a number of separate special disciplines, notably anatomy, physiology, anthropology, psychology, genetics, sociology, economics, history and biostatistics" (Unpublished course lectures, cf. also, Pearl, 1924, 1935b). This synthesis resolves itself into an integration of the multiple facets of man (which appear from the analytic procedures of the above disciplines) among themselves and with the basic biological constitution deriving from his zoological rank. This process of integration has already been successfully initiated for certain aspects of man's manifold personality. We need only recall, as an example, the work on somatological constitution which attempts to measure the degree of consilience between the anatomical, physiological, and psychological personalities of man. Not so advanced is that part of human biology which refers primarily to social behavior. Responsible for this lag in the development of human social biology is the lack of a satisfactory and productive approach to the problem of determining the elements, biologic or otherwise, of social behavior. A discussion of this question is the primary purpose of this paper which is to serve also as an introduction for the future presentation of the results of some original investigations on the subject.

ANIMAL SOCIAL BEHAVIOR AND INSTINCTIVE BEHAVIOR OF MAN

Darwin's *The Descent of Man* closed with the phrase "Man still bears in his bodily frame the indelible stamp of his lowly origin" (p. 619). It is difficult to realize now that this thought was set down in defiance of ideas prevalent less than 70 years ago. That man is an animal and as such alone possesses certain attributes and behavior characteristics is a concept

accepted today so generally by students of biological sciences that its restatement acquires the quality of redundancy. Yet its explicit statement is less than a century old and all its implications, particularly those referring to man's social behavior, have not been at all seriously explored. This is not surprising because there is always a lag between the enunciation of a principle and its general application. It is remarkable however that the disregard for the biological foundation of man's activities should include also a neglect of the importance of his morphological and physiological characteristics as factors in behavior. A glance at the textbooks of sociology is sufficient to show that very few give more than one or two paragraphs to the so-called 'drives' or 'urges', and generally no mention is made of the fact that man's activities are subjugated to the requirements of his physical organism. These needs are real and tangible and are altered only in a minute degree if at all by the superorganic manifestations. Satisfaction of these needs or biological urges is the only categorical imperative for the existence of man as an individual or as a species. When it is a matter of group survival, the importance of religious principles can give way to Henry of Navarre's answer that Paris was well worth a Mass. And when slow starvation seems inevitable, even the so-called filial and parental instincts are repressed by the hunger which seeks alleviation in cannibalism as Gantt (1937) and others have reported from Soviet Russia. It is recognition of these fundamental facts that gives value to the population theory of Malthus, the historical materialism of Marx, and the psychoanalytic approach of Freud.

In common with all living creatures, man demonstrates those qualities which Pearl has summed up as follows: urge for individual survival, urge for reproduction, variability. The multitudinous manifestations of these qualities or their relationship with the differences in structure and function of the many kinds of living creatures cannot be discussed here. Based on the elements in common, one approach to the study of human social biology is that of animal experimentation and observations.

It is this approach which has been responsible for much of the recent progress in physiology, medicine, psychology, genetics and other branches of sciences. For the study of human social behavior, however, it has not proven of equal value, although since animal behavior has more and more occupied the attention of biologists precise observations have been recorded that are unquestionably useful for the student of social sciences. From the classical observations of Espinas (1878) to the more recent ones of the late William Morton Wheeler (1928b, 1933, 1937) there is available information that is important probably not so much for its immediate usefulness in the solution of human social problems but mostly as a means of understanding clearly what they are all about.

Wheeler, for example, in the numerous publications covering long years of study, has critically examined his own observations and those of others on the social insects, the ant in particular, to arrive at a comprehensive portrayal of their social behavior. There are many deductions important for our purposes to be made from his observations. The tendency to sociality is manifest by these insects in a manner weaker but more constant than the sex and hunger cravings. Society is organized as a mother-family pattern with integration around the original nest-mother varying in degree. In some cases any invader who successfully ousts the

nest-mother is accepted without overt hostility, in others it is repelled. The complexity of the social organization varies and with it the differentiation between castes, both morphologically and functionally. The more complex is the social organization, the more complex appear to be the societal functions and greater is the differentiation between the castes. The differentiation of the castes may be slight and conditioned by the environment, or it may be striking and presumably genotypic in nature.

If we pass from insect societies to the less extensive observations on sub-human primates, utilizing Zuckerman's (1932) report based principally on the Hamadryas baboon colony of the London Zoological Garden, the following facts seem important. The sexual element, both homosexual and heterosexual, prevails in the social interrelations. The family unit is a product of the dominance of a male over the female and over other males. The family integration depends entirely upon the continued dominance of the over-lord. Societal functions vary within limits and are characterized mostly by their absence. What there are result from a series of relationships of a dominance-submission pattern between the individuals.

As one ponders these observations and notes the numerous points of resemblance between the overt social behavior of these animals and that of man, it is easily understood why from time to time attempts are made to describe the superorganic manifestations of man entirely in terms of animal behavior. The theory of organic evolution has given a stronger impulse to this way of thinking and there are a number of students of the social sciences who have ventured to trace the evolution of social traits. Kropotkin (1902), for example, studied the phenomenon of social coöperation and its importance in the rise and decline of animal and human societies. He arrived at the conclusion that coöperation is one of the most important if not the only factor responsible for the survival of the group. The fact that Kropotkin's political opinions prejudiced some of his interpretations based on good, bad, and indifferent observations has perhaps worked against an adequate appreciation of his work. Westermarck's (1903) study on the history of human marriage is another example of the attempt to chart the course of the evolution of social institutions. He emphasizes in particular the stable predominantly monogamous attachments said to be observed in subhuman primates and concludes that such is the primeval pattern of human marriage. In later studies, following criticism of the sources of his information, this opinion was somewhat modified. All these and similar studies, aside from the doubtful quality of the observations on which they are based, have little value except as hypotheses which may or may not be useful. This is so because of our ignorance regarding the processes which have led to the human branch of the animal trunk.

From a recognition of the characteristics of the behavior of other animals is derived the concept of instincts on which is founded the social psychology of McDougall (1921). For him, the instincts are native propensities, innate or inherited tendencies of the human mind; variable according to individual and group but probably common to men of all periods and places. They develop under the guidance of intelligence and are modified by the state of culture. He notes in particular, as Russell (1934) has, that instinct is not a purely mechanical process but in its action involves cognition, affection and conation. He recognizes primary and

secondary instincts, the former being characterized by their presence in higher animals and the possibility of appearing in an exaggerated form. The list of instincts which he gives is modified from that of James, and each instinct is accompanied by an emotion by which it is manifested. The instincts mentioned include those of flight, with fear as its corresponding emotion; repulsion, and the emotion of disgust; curiosity, and the emotion of wonder; pugnacity, and the emotion of anger; parental, and the tender emotions; self-abasement and self-assertion, and the emotions of subjection and elation. Reproduction, gregariousness, acquisition and construction are among the less clearly defined instincts, and apparently have no emotion attached to them, according to McDougall. The action of these instincts on social phenomena and individual social behavior is illustrated in a variety of ways. The size of population is regarded as a function of the reproductive and parental instincts. These instincts can be inhibited by intelligence and therefore social sanctions are often necessary to support them. The parental instinct is also manifest in the benevolence shown to the lower classes, aid to the oppressed, etc. Another of the instincts of prime social importance is that of pugnacity because it assures the selection of the fittest, either individual or group. Greater pugnacity is associated with other qualities which lead to a higher degree of civilization. Moreover this instinct is held to play an important rôle in the development of penology and is finally altered into the tendency to emulate. The gregarious instinct has had its primary importance in keeping men together, and therefore provides the stimulus for the development of society in its modern aspect. Religion in its more primitive form, according to McDougall, is a product of fear, curiosity, and subjection. In its higher form it derives also from tender emotions, awe, and gratitude. The instincts of acquisitiveness and construction are not directly social but as the words imply are involved in many social activities.

McDougall's social psychology, only cursorily outlined here, represents the most elaborate attempt to explain social behavior in terms of elementary biological factors. It falls considerably short of the mark and this failure demonstrates the essential difficulties and limitations of this direct biological approach to the problems of human social behavior. It is not the place here to enter into the controversy regarding the existence of instincts. The extreme position taken by Watson (1930) on this subject is curious, since he is forced to admit the inheritance of physical traits but not of psychic ones, and yet observes unlearned tendencies of a decidedly psychic nature. The whole controversy is not very inspiring and apparently useless, as any one can see from the sober factual study of the subject made by Russell.

There is another weakness in the position taken by the school of instinctive social behavior. In the first place it has not been shown that all the social reactions of animals including man are instinctive in the sense defined by McDougall. Wheeler's (1933) words on this point, when he discusses the behavior of the solitary bee who builds the cell and provisions it for the larva which is as yet still undeveloped in the egg, are worth remembering.

"All these apparently purposeful arrangements are indeed wonderful, but so are the intricate devices which insure the nourishment and protection of the embryo in the seed of the higher plant or of the foetus in the uterus of the mammal, and yet neither the botanist nor the embryologist imagines that he is

gaining any insight into such physiological phenomena by attributing them to 'maternal instinct'. The term is quite as useless when applied to the behavior of the mother wasp, bumble-bee or ant, which is acquainted with her eggs and larvae. We may assume, at least till proof to the contrary is forthcoming, that the eggs and young larvae 'impose themselves on the parent', that is, act as stimuli which elicit the nursing responses, because they emit specific agreeable odors or secretions that makes these responses irresistible" (pp. 148–149).

In the second place, it has never been made clear where and how McDougall obtained the evidence of the existence of those human instincts that he mentions. Certainly not from animal observations because the list would be different, as is evident from the cited studies of Wheeler and Zuckerman. And certainly not from human observations since what specific ones there may be cannot as yet be discerned from the behavior characteristics imposed on man by his environment, social or otherwise.

These are defects of a theoretical and formal nature. The main shortcoming of McDougall's social psychology is that it does not establish in any way the continuity between postulated instinct and the concrete social phenomenon. Even if the list of given instincts is correct the way is not possible except by speculative acrobatics to follow the development of the reproductive instinct, or the instinct of acquisitiveness, or what have you, into the various forms of the institution of marriage or that of prostitution, for example. This illustrates the fundamental limitation inherent in the attempt to reach an understanding of human social behavior simply from the study of animal sociology. Such a limitation derives from the differences between man and other animals. Man possesses the ability to formulate and express abstractions. He is, as Pearl (1937a) aptly says, a time-binder, an organ-adder, and an environment-changer. He not only responds to internal physiological stimuli and to the external physico-chemical ones, but he also thinks and talks about them and in consequence initiates a whole train of factors which go under the name of social conditioning. By inductive reasoning starting from the observations of animal social patterns it is impossible to construct anything but the vaguest generalities about the biology of man's social actions. It is necessary instead first to disentangle from the complexity of the relations between man and other men the stable elements, the manifestations of his human nature which include those derived from his "lowly origin".

THE INDIVIDUAL AND THE GROUP

Menenius Agrippa, as Livy relates, convinced the seceding Roman plebeians of the justice of the caste system by drawing an analogy between the functioning of the individual and that of the social group. Many centuries later Herbert Spencer drew the same analogy in attempting to construct a theory of social transformation parallel to that of organic evolution. Spencer's illustrative analogy as well as the inferences he derived therefrom were to leave their mark not only on human sociological thought, expressed by what Sorokin (1928) denominates the bio-organismic school, but also on social psychology and animal sociology.

Society, notes Spencer (1921), is an organism composed of individuals that like cells are interdependent in activity and can survive the destruction of the total organism. The cellular structures of the social organism are also differentiated relative to form and function, and the organism like the individual society passes through the phases of growth, decline, and death.

Such an analogy is somewhat superficial and probably its main value is a political one, in the sense used by Agrippa. The general applicability of this concept to concrete phenomena apparently depends in great part upon the degree of functional and structural homogeneity of the elements composing the organism and the kind of interrelationship which unites them. In the realm of human social behavior this principle could perhaps be utilized best in the study of groups composed of fairly homogeneous individuals with well-defined functions and linked together by strong and endurable ties.

The influence of Spencer's concept is seen in the development of the theories of "crowd" or "mob" psychology and of social emergence. These theories assume, to a varying degree, that the social group is something that emerges from its component members, in the same fashion that the individual organism is something more than and different from the mere summation of its cells. Le Bon (1896), for example, believes that when an association is formed, new characteristics are acquired different from those of the single individual; a collective mind is created which feels and produces actions in a manner which can be totally unlike that of the members of the group. This process is essentially attributed to contagion or imitation and suggestion. Since this process is essentially attributed to contagion or imitation and suggestion it follows that actually the crowd acts according to the ideas or feelings of those individuals who are being imitated and as a group it must be held together for a time sufficiently long to allow the suggestions to operate.

More cautious, although not fundamentally different, are the views of Wheeler (1928a) who discusses this matter primarily from the standpoint of social evolution. For him the formation of the group gives rise to emergence which is "*a novelty of behavior* arising from the specific interaction or organization of a number of elements", and besides is "neither the manifestation or unveiling of something hidden and already existing, . . . nor some miraculous change" (p. 14). In justice to this author, it must be added that he does admit that the emergent pattern of an association depends also upon the functional peculiarities of the component organisms, a statement logically incompatible with the previous definition of emergence.

The main factual foundations for the maintenance of a theory which postulates that a society is an emergent entity, derive from the observations on the group behavior, especially of groups of a temporary nature, apparently so divergent from the usual behavior of the individual participants. Massacres, lynching and similar violent and sanguinary actions on their face seem to result from the transitory association of erstwhile law-abiding, peaceful people. From such facts it would seem justifiable to deduce that the element of association is responsible for the group behavior. It also would appear justifiable to assume that imitation or contagion, as Le Bon prefers, can be a stimulus for the group action, especially since students of animal behavior (Espinas for example), have long ago described its mechanism in other animals. In the same way, suggestion might well be considered an important factor from what the psychiatrists have taught us.

The theory would seem plausible except that further analysis of this type of phenomena reveals that the apparently peaceful, law-abiding citizens who participate in a lynching, let us say, have peculiar characteristics which distinguish them from other peaceful, law-abiding citizens.

Sighele (1895), who has discussed the matter in detail from the standpoint of criminology, ably points out this fact when he compares the differences in the composition and in the action of the mobs which revolted in Paris in 1750 and 1793. From such historical observations and the examination of criminal case records he is led to conclude that neither imitation nor suggestion alone can be held responsible for the criminal acts of a mob, in particular of one formed at short notice. Rather these acts appear to be due to the psychic and physiologic status of the members of the group at the moment, and their interrelations with particular reference to the domination of some individuals over the others. The main facts about the usual form of Negro lynching by whites indicate that this view is substantially correct. The following conditions must be satisfied before a lynching is possible: (1) The subject to be lynched must be a Negro whose alleged victim is white; (2) the crime must be committed where the sentiment against Negroes is strong; (3) the lynching mob must share the sentiments of the locality; (4) none of the dominating personalities of the group is opposed to murdering a Negro; (5) the dominating individuals of the group are able to arouse the emotions of the uncertain majority; (6) the members of the mob are physically capable of committing the lynching. Unless such a combination of elements is formed there will be no lynching, hence lynchings are confined to certain localities and occur sporadically. A similar analysis of other group activities will lead to the generalization that any social phenomenon results from a particular type of interaction between certain individuals and this interaction depends upon the kind of individuals composing the group.

In view of these considerations it is possible to define a human social group in general as being an association of persons who temporarily or permanently possess one or more attributes, functions, or characteristics in common, no matter how much they may differ otherwise. A nation, for example, is an aggregate of diverse individuals who either through accident of birth, descent, migration, capture, etc. are socially constrained by a set of laws and mores. A scientific society, instead, is composed of diverse individuals who are overtly interested in some branch of science and selected either through merit, or simply through the payment of certain fees. A family, on the other hand, usually consists of two types of individuals, those who form the marital union, the nucleus of the family, and those related to each or both of the partners through inheritance or marriage. Thus it is seen that an individual may participate simultaneously to a variety of groups, and his inclusion into a group is due to a multiplicity of causes, ranging from mental qualities to the accidental meeting of an ovum and a spermatozoa.

The qualities of the individuals comprising the group determine the group, and conversely the group or rather the other individuals exercise a certain amount of influence on the individual. In general, emphasis by the sociologists has been placed on the effect of "society," the "laws" and the "mores" on the individual. But this "society" is only all the individuals, the "laws" and the "mores" are products of all the individuals, and all of these are to some extent influenced by the presence and action of any particular individual. The discussion of the effect of family training on the child implicitly assumes that the latter is a plastic thing which is there to be modelled at will by the parents. Such an image is far from reality because it does not account for the unlearned qualities of the child, nor for

the fact that the child's behavior may affect the parents in such a way that they are unable to carry out a program of instruction or discipline. The failure of the penal systems of the past and present, as the observations of the Gluecks (1937) show, are again evidence of the inability to take into account the individuals on whom the system is to be used. In the light of the discussion so far the conclusion seems warranted that the study of the social organism reduces itself to a study of the individual, and the elements that enter into the interaction between individuals.

STABLE FACTORS IN SOCIAL RELATIONS

One of the most important characteristics of man which he shares with all living creatures is variability. It is manifest in all his traits, morphological, physiological and psychical including sociological. There are however limits to this variability due to the innate or acquired constitution, to the environment, or to both. To exceed, if possible, or to approach the limits of the range of variation places the individual in a position apart from the remainder of his group. If in an individual the body temperature exceeds 99 degrees F. he is no longer among the persons in good health, if it exceeds 104 degrees F. he risks being no longer among the living persons. Similarly, in our civilization if a man commits murder and is caught he loses certain civic privileges and is no longer classed among the law-abiding persons. If during a war a man should refuse to participate he also loses certain privileges and is no longer a patriotic citizen. In these last examples the individual has exceeded the permissible limits of the range of variation of those specific social actions. The limits to individual variability in action of direct or indirect social import constitute the material that forms the object of sociological investigations. Laws and written codified regulations are one type of social factors that determine the permissible range of variation in the relations between persons. They represent the highest development of rationality and rationalization in the attempt by the group to limit individual social actions. They are, however, only the specific manifestations of the class of phenomena which goes under the name of customs, or mores, or folkways, the universal limits of conduct imposed on the individual by those composing the group. They are responsible for the peculiarities of man's social behavior which in its uniformity, persistence, and mostly in the apparent lack of conscious effort resemble closely that of other animals. This fact has not escaped Sumner (1906) who has given us what is probably the most admirable description of the mores. He says (p. 4):

> "The folkways, therefore, are not creations of human purpose and wit. They are like products of natural forces which men unconsciously set in operation, or they are like the instinctive ways of animals, which are developed out of experience, which reach a final form of maximum adaptation to an interest, which are handed down by tradition and admit of no exception or variation, yet change to meet new conditions, still within the same limited methods, and without rational reflection or purpose."

And further on, in summarizing the whole varied forms by which the mores operate in the relations between the individual and other individuals Sumner notes:

> "The relations of men to each other, when they are carrying on the struggle for existence near each other, consist in mutual reactions (antagonisms, rivalries, alliances, coercions, and coöperations), from which result societal concatenations and concretions, that is, more or less fixed positions of individuals and subgroups towards each other, and more or less established sequences and methods of interaction between them, by which the interests of all members of the

group are served. The same might be said of all animals. The social insects especially show us highly developed results of the adjustment of adjacent interests and life acts into concatenations and concretions. The societal concretions are due to the folkways in this way,—that the men, each struggling to carry on existence, unconsciously coöperate to build up associations, organizations, customs, and institutions which, after a time, appear full grown and actual, although no one intended, or planned, or understood them in advance. They stand there as produced by 'ancestors'. These concretions of relation and act in war, labor, religion, amusement, family life, and civil institutions are attended by faiths, doctrines of philosophy (myths, folklore), and by precepts of right conduct and duty (taboos)." (Pp. 34-35.)

The folkways are concerned in all aspects of the social activities and as Sumner shows they take different forms according to time and place. Customs of dress, eating, and amusements, morals, religious rituals, types of government are all manifestations of the mores. Taking as point of departure the concept of the mores and the varied manifestations of the folkways, a logical sequence seems to be that of inquiring into the factors present in the single individuals and the group which make the mores different in their form and intensity; or in other words, to investigate the circumstances responsible for a reduction or increase in the range of variation of individual social actions. Sumner has not attempted this inquiry; other investigators have instead sought the origins of the mores or their manifestations, some with the help of animal biology as has been noted, many with an assortment of preconceived notions; still others in considering the social institutions and laws have overlooked the unconscious element in the formation of the original mores. Among students of social sciences, Pareto has made the most successful effort to deduce from the conglomeration of social phenomena certain basic elements of social interrelation. Since his work represents from the standpoint of this discussion the most important contribution on the subject it deserves to be given as thorough an appraisal as is possible in the brief space available. Pareto (1923) begins his study of social phenomena by making a distinction between logical and non-logical actions, defining the first as those actions that experimentally, and for the individual who commits them, tend toward one and the same objective. Non-logical are those actions which are committed with a purpose not experimentally or factually evident. This classification depends then entirely upon the degree of knowledge that is possessed at the time about a particular activity. Thus, fifty years ago to shoot a cannon as a means of dissipating yellow fever would have been considered a logical action since it was a consequence of the supposed knowledge of the day; now it falls under the heading of non-logical actions. On the other hand, according to the lights of fifty years ago, the idea of flying with a heavier-than-air machine was absurd; today it is not. The concept of relativity which he introduces in the definition of these terms as well as in others he uses is one of the most significant and as yet so little appreciated qualities of Pareto's work.

In a manner reminiscent of Sumner, Pareto emphasizes the non-logical aspect of social actions. With extensive and detailed historical facts he gives convincing proof of the preponderance of non-logical actions among societal manifestations. He differs in one respect from Sumner because the latter postulated utility as a factor in the origin of the folkways, while Pareto sees no good reason for the interjection of this element.

Having demonstrated the importance of non-logical actions in social phenomena Pareto proceeds further to note that the action of man is accompanied by an explanation intended to establish the

rationality of the act. In this, man makes use of vague undefinable terms and the clichés of his civilization. The failure to recognize that the logical or pseudo-logical explanation, the theory, does not always precede the action has caused confusion in social sciences, Pareto observes, since a great deal of research has been wasted on trying to show that the action is a logical manifestation of the theory.

Now we come to the most important part of Pareto's system. From the examination of the phenomena he finds that all social manifestations consist of a variable and incidental part (the rational explanation) and a constant element (the motivator of the action). This fundamental psychological factor is neither an instinct nor a sentiment. Sorokin thinks it has the properties of Allport's prepotent reflexes but Pareto explicitly leaves the matter of its nature to the psychologist. This constant factor is given the name residue, the variable one is called derivation. The derivations include all those verbal arguments used to support a thesis: authority of person or thing such as the Bible; metaphysical and legal entities—nature, democracy, social contract, etc.; verbal proofs such as flat statements, or arguments about individual or social utility. Illustration of the reasoning adopted in analyzing a social phenomenon is given by the following example. Baptism is a Christian rite to cleanse the original sin and water is used for the purpose. Water was also employed in the Roman ceremonies of purification. Thus one could say that in purification rites water is employed because of the association between water and cleansing. Additional information on the subject reveals that besides water other things such as blood, mud, and sticks are used in purification ceremonies. The means employed and explanations advanced for their use are the variable and incidental elements of the phenomenon according to Pareto, the constant factor is the complex of feelings and beliefs which assumes that the lost spiritual integrity of the individual can be restored by the combination of certain practices.

Pareto arrives at six broad categories of residues. The first is that of the residues of *combinations*. Under this heading he includes the constants of the whole series of social actions that results from the mental processes of associations of entities—things, persons and words—with no logical connections between them. Widespread social activities are included here such as those which derive from luck—lucky numbers and persons, the evil eye, etc.; theogony and mythology; political slogans; religious rituals. These phenomena are the products of the inventiveness of the human mind without the benefit of intellectual superintendence. The second category of residues are those to which Pareto has given the name *persistence of aggregates*. These concern those actions which demonstrate a desire to maintain a *status quo*. They are probably best illustrated by the show of attachment to one's birthplace, home, group, ideas, ways of living, or one's dead. Conservatism could probably be another name for the tendency manifested by these actions. A third class contains the residues of *manifestations* and includes the constant element of the external acts derived from emotions and sentiments. Parades, processions, all the forms of man's religious, political and patriotic exaltations are grouped in this class. In the fourth class of residues, those of *sociality*, are included the constants of those non-logical actions which serve as the rules for the intercourse between persons. The need for conformity, pity for some and cruelty directed against others, benevolence for the weaker, loyal-

ty to the superior are the sentiments associated with the type of actions included here. Another category includes the residues of *integrity*. These are manifest by those actions taken in defense of the so-called interests; property, family, country, etc. They are evident in such contradictory activities as the fight for equality and that against revolutions. This class of residues contains therefore those that stimulate active movements while those included among the residues of persistence of aggregates stimulate a passive resistance to the same type of situation. The sixth and last class of residues is the *sexual*. In the formation of this class Pareto demonstrated unusual acumen since he included not only those activities which are the direct consequence of the expression of the sexual urge but also those which represent the sexual inhibitions, repressions, and perversions, that is, such phenomena as morality, asceticism and flagellation.

This is the nucleus of Pareto's system. He believes that these residues are present in every individual but with different intensity. The social actions and interactions observed in a society result from the variation of the strength of the several residues among the members of the group.

Without doubt the system of classification by which he arrives at his residues lacks an organic basis. But it is entirely phenomenal and avoids the futile attempts to separate nature and nurture, unlearned, and conditioned factors. However, before this classification can be of practical utility or acceptable a quantitative analysis must demonstrate more precisely than Pareto has done how the categories are formed and if they are at all comprehensive. That this system could be improved upon was realized by Pareto himself if not by his devoted desciples, who, in general, have limited themselves to loud hosannas about the master's work.

It is not actual classification of residues which is of importance but rather that Pareto has shown how the complexity of social phenomena could be reduced to a point where it seems possible to assume that by means of animal observations and experiment, a clearer understanding of the fundamental factors of human social behavior can be obtained.

As observations on lower animals clearly show, the stable elements in their social behavior can be objectively studied and the search for causal factors therefore theoretically rests on a foundation of precise knowledge or that can be made so. The stable elements of the social activities of man, on the other hand, cannot be always studied with the same degree of precision because in the majority of cases they are not clearly to be separated objectively from the variable and incidental elements. This fact is responsible for the poor showing of those attempts to apply directly to man the observations made on animals. It would seem that Pareto has shown that it may be possible to eliminate the incidentals and to arrive at a view of the fundamental characteristics. If he is correct then it means that for the study of man's social behavior, the same methods that have proved so efficient in other branches of human biology can be utilized.

CONCLUSION

In sum, the study of human social behavior is here envisaged along the lines, for example, of physiology where the physician or the physiologist makes his observations on man, but from animal experiments derives the knowledge necessary to elucidate the observations. Already there exists valuable work on animal social experimentation such as that of Allee (1931, 1938), for example, but just

as is the case relative to purely observational work on animal sociology, such knowledge for the time being cannot be extended to man. This is so because the students of human behavior have yet to provide observations adequate for the coöperative enterprise. From a consideration of the progress in other branches of human biological sciences there is reason to believe that only when this coöperation is achieved will it be possible to reach a more definite understanding of man's social activities. From all that has been said before it appears that the effectiveness of the efforts directed toward this objective will depend in great part on how clearly it is realized that the study of human social behavior must be based on concrete elements—on the individuals who compose the group and on their mutual relations. In other words, the social activities of man are not to be looked upon as configurations of geometric points, or as movements of automatons, or simply as the behavioristic responses of animals but instead they are to be regarded as the manifestations of living creatures whose mutual relations reflect to a greater or less degree, at one end and at the same time, their powers of rationalization and their biological constitution. To deduce from these phenomena, accepted in all their complexity, the stable elements of the relations between man and other men is the immediate problem which faces the student of human social biology.

The author takes this occasion to express his deep feeling of gratitude to Dr. Raymond Pearl whose encouragement, advice and criticisms have been of incalculable aid in the preparation of this article.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that The Quarterly Review of Biology *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* The Quarterly Review of Biology, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

Charles Darwin: *A Portrait.*
By Geoffrey West. Yale University Press, New Haven. $3.50. $9\frac{1}{4} \times 6\frac{1}{4}$; xiv + 359 +8 plates; 1938.

For all too long Darwin's life has been overshadowed by his works, and they in turn have been too largely dominated by one book. Because the *Origin of Species* has affected scientific and philosophic thought more than any other biological book has ever done, an unmerited condition of relative neglect has been the fate of *The Descent of Man*, *The Variation of Animals and Plants under Domestication*, *The Expression of the Emotions in Man and Animals*, *Coral Reefs*, *Volcanic Islands*, *The Power of Movement in Plants*, *The Formation of Vegetable Mould through the Action of Worms*, and the *Voyage of the Beagle*, to mention only a few of his less well-known writings.

The biography of a great man must inevitably consist of much more than a mere bibliography of his writings, but the literary output of a man like Darwin must loom large in any account of his life. Darwin was a man of extreme modesty—even on the celebrated occasion on which he gave his new theory to the world, when Wilberforce and Huxley locked horns over it, he absented himself in order to avoid the possibility of having to defend it extemporaneously before the intellectual lights of Great Britain. To appear in public was extremely distasteful to him. He was a great prophet, but he spoke through his pen. To understand his message one must be familiar with his writings.

But the man himself was something quite distinct from his message. To understand the man it is necessary to consider his ancestry. The author of the present work begins with the two grandfathers, Josiah Wedgwood and Erasmus Darwin, and the first quarter of the book consists mostly of a scholarly presentation of the biography of these two men and their families, particularly Robert Darwin and Susannah Wedgwood, the parents of the propositus.

The life story of Charles Robert Darwin *sensu stricto*, occupies something more than half the book. It is a stimulating and encouraging story, for Darwin in his youth gave little evidence of the genius that characterized his progenitors and which was later to reach its full fruition in him. An impediment in his speech made it difficult for him to meet other people. At Cambridge, where he attempted to take up medical and ecclesiastical careers by turns, he was a failure—and then came the opportunity to circumnavigate the globe on the *Beagle*. At first he met with opposition from his father, who underestimated his son's ability, and from Captain Fitzroy, who did not like the shape of young Darwin's

nose, but the latter's persistence, fortified by that of his Uncle Josiah Wedgwood, Jr. finally carried the day, and the course of scientific thought began to be diverted.

The author of this biography confesses that he undertook the work with a personal prejudice against Darwin, because the latter was such a perfect embodiment of Victorian Morality, but that as the work proceeded he developed a genuine affection for the man. The reader will readily understand this, because as he proceeds he will realize that Darwin was not only one of the most significant characters in the recent history of civilization, but also one of the most appealing. The author has done his work well both as history and as literature, and not its least interesting feature is the glimpses that he gives us of some of Darwin's contemporaries, such as Lyell, Hooker, Wallace, etc. The bibliography covers eleven pages and the index seven and there are eight plates.

FOSSIL ANTHROPOIDS OF THE YALE-CAMBRIDGE INDIA EXPEDITION OF 1935.
By William K. Gregory, Milo Hellman and G. Edward Lewis. Carnegie Institution of Washington, Washington, D. C. \$1.00 (paper); \$1.25 (cloth). 10 x 6⅞; 27 + 8 plates; 1938.

The findings of the Yale-Cambridge Expedition to the Siwalik Hills are described and compared with other anthropoid material. An excellent study has been made of teeth measurements and many fine plates depict these important structures. The general results are as follows:

> In closing, we deem it important to emphasize the following facts: (1) that the extinct anthropoid apes ranged over an enormous area in the eastern hemisphere—from Spain in the west to India and China in the east, and southward from Egypt to South Africa; (2) that the group as a whole was exceedingly variable, at least in the details of the jaws and dentition; (3) that while the Siwalik genus *Ramapithecus* and the South African *Australopithecus* were still simians by definition, they were almost at the human threshold, at least in respect to their known anatomical characteristics.
>
> Nor can we find any convincing evidence that the peculiar features of the teeth of *Sinanthropus* and later hominids (such as irregular folds and wrinkles of the enamel surface of the molars, taurodontism of the molars, the shovel-shaped form of the central upper incisors, the lowness and bluntness of the canines, etc.) are ever anything more than specializations, of later date than the opposite characters found in the upper Tertiary anthropoids of the Siwaliks.

GENETICS

ERBMATHEMATIK. *Theorie der Vererbung in Bevölkerung und Sippe.*
By Harald Geppert and Siegfried Koller. Verlag von Quelle and Meyer, Leipzig. 16 marks (paper); 18 marks (cloth). 9½ x 6¼; viii + 228; 1938.

This is an outline of some of the methods of evaluating quantitatively the genetic factors in human inheritance. The author first describes the probability mechanism concerned in the computation of the simple Mendelian ratios and then proceeds to develop the formulae necessary for the study of more complex situations such as occur in man. He illustrates the method of estimating the frequency of a genetic factor in a random population, the estimation of the number of factors involved and of their allelomorphic behavior, linkage, etc. He discusses also and at length the different methods of analysis according to the selection of the sample: family, sibs in general, twins. From the standpoint of theory this book provides useful and necessary information for the student of genetics. Since the formulae are derived by elementary methods, no great knowledge of mathematics is necessary to understand them. However, the applicability of most of the formulae to concrete problems of human genetic research is somewhat limited.

HEREDITY. *Third Edition.*
By A. Franklin Shull. McGraw-Hill Book Co., New York. \$3.50. 9 x 6⅛; xvii + 442; 1938.

The current edition (second edition reviewed in Vol. 7, No. 1 of the Q. R. B.) of this work is a worthy successor to its predecessors. It differs from them chiefly in the order in which the contents are presented, the inclusion of an appendix of thirteen pages containing a compact treatise on biometrical methods, a bibliography of eight pages of recent literature, and in being more or less completely re-

written in the light of the advance made by the science of genetics since the last edition appeared. The index has been expanded to eighteen pages. A very useful standard text.

Die Vererbung der Geistigen Begabung.

By Friedrich Reinöhl. J. F. Lehmanns Verlag, Munich and Berlin. 6 marks (paper); 7.20 marks (cloth). (Outside of Germany 4.50 marks (paper); 5.40 marks (cloth)). $8\frac{3}{4}$ x $6\frac{1}{4}$; 280; 1937.

An interesting treatise, primarily intended for the general reader, but useful to the specialist because of its documentation, on the inheritance of mental endowment. The author discusses the general fundamentals of genetic theory; techniques of genetic investigation in man; the concept and measurement of intelligence in man; the relations of heredity and environment; mental disease; and the mental endowments of races. For data he leans heavily on American recruiting statistics and German material.

Mule Production. *U. S. Department of Agriculture. Farmers' Bulletin No.* 1341.

By J. O. Williams, Revised by S. R. Speelman. Government Printing Office, Washington, D. C. 5 cents. $9\frac{1}{2}$ x 6; 28; 1938 (paper).

A brief treatise on the production and care of mules. Being intended strictly for practical use, it contains no discussion of why some mules are fertile and others are not. A surprising feature brought out is that a good jack for breeding asses is not necessarily a good jack for breeding mules.

GENERAL BIOLOGY

Mountains of the Moon. *An Expedition to the Equatorial Mountains of Africa.*

By Patrick M. Synge. E. P. Dutton and Co., New York. \$4.00. $8\frac{1}{2}$ x $5\frac{3}{4}$; xxiv + 221 + 93 plates + 1 map; 1938.

Flying Fox and Drifting Sand. *The Adventures of a Biologist in Australia.*

By Francis Ratcliffe. With an Introduction by Julian Huxley. Robert M. McBride and Co., New York. \$4.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; [14] + 341 + 32 plates + 4 maps; 1938.

These two books bear eloquent witness to the fact that the days of Gilbert White, William H. Hudson, and James H. Ferriss are not yet over. The writer who can command a good literary style in which to recount what he has seen, and who is gifted with powers of observation to see what others do not, whether in his own back yard or in the uttermost parts of the earth, need never lack an audience.

Much has been written in recent years about the fauna of the Dark Continent—chiefly about the mammalia, but the flora and the invertebrate fauna have been more or less neglected, although they are equally remarkable and differ just as widely from those of the northern hemisphere. Mr. Synge's work will go far toward remedying that lack. The region about Uganda, of which he writes, is a strange country. Here is the home of the hyrax, that pocket-sized edition of the elephant, whose raucous ululation paralyzed the author every night until he learned what was making it; the honey plant which is merely a lobelia that has acquired the habitus of a yucca; the succulents which are not related to the cacti at all, but which have adopted the same method of fighting the relentless drought; the stone flowers, which are neither stones nor flowers. With two companions the author ascended to an altitude above which no man had ever stood before him on the slope of Mt. Ruwenzori, the snow capped peak on the equator whose glaciers feed three rivers each of continental dimensions, each seeking a separate sea, and he penetrated a forest of tree heather at an altitude of two and a half miles to look upon the silent waters of the Lake of Death, where his native porters refused to accompany him, out of fear of the unknown.

The book has been well illustrated by the brush and camera of the artist Stuart Somerville. A critic has said that the test of a painted landscape is its ability to evoke a desire to go walking in it. Judged by this criterion the illustrations pass with flying colors. The scenery of Ugan-

da is not unlike that of our own southwest, with its granite peaks and arid deserts, except that the senecios take the part of the Joshua trees and chollas, and the versatile lobelias imitate the saguaros and the biznagas.

Mr. Ratcliffe's book deals with an even stranger region—the land of the koala and the dingo, the wallaby and the platypus, the emu and the kookaburra. Although sent thither to investigate specifically the damage done by the flying foxes, which are fruit-eating dog-faced bats of considerable size (wing spreads of five feet are not unknown) and wind erosion of the soil (for Australia has a dust bowl problem even more serious than ours) he took notes on everything he saw, the jungles of tree ferns and lawyer vines and stinging trees, the eucalyptus trees of the plateaus, the evergreens of the mountains, together with their living inhabitants. He also tells us much of a personal nature about the people whom he met—the farmers, the cattle raisers, the lumber jacks, and their families breaking virgin ground for the spread of civilization—in fact, his delineations of character are on a par with his descriptions of scenery. It is difficult to imagine anyone reading this book and not wanting to go to Australia.

A Prairie Grove.
By Donald C. Peattie. Simon and Schuster, New York. $2.50. 8 x 5½; 289; 1938.

This long-range view of the early flora and fauna on the American continent approaches the proportions of an epic. The author's lyrical descriptions of the forest, the rivers, the rocks, the prairie, the grasses, the buffalo herd, the wolf and the shrew make him a master of "prose poetry." The characteristics and habits of the animals and birds in their varied and seasonal environments are described as only a keen naturalist and poet could describe them. Into this background are projected the Indian aborigines, the French explorers, priests and settlers. Later the New England pioneers are brought into the story. They were imbued with a "vision of empire" and ventured into the western wilderness that is now Illinois. The effects of the crude life and the new country upon these stalwart plainsmen and their companions are shown by drawing an idealized picture of the Goodner family. The individuals selected are perhaps symbolic of those who pioneer—their reactions and developing abilities or frailties are treated with amazing insight and understanding.

But Mr. Peattie is certainly at his best in describing the primeval prairie and the natural beauty of "the state that is shaped like an arrow-head." His esthetic appreciation of this land before "the grass at last learned obedience," and before civilization encroached upon it, is apparent in every sentence.

The book is an unusual combination. It will appeal to lovers of fine English, and will also prove a delight to those who are interested in the natural history of the Middle West. It merits many readers, and a permanent place among the classics of America. Fifteen pages of "Bibliography of Sources" at the end of the book give additional evidence of the historical and scientific data used by the author.

General Biology. *A Textbook for College Students.*
By Perry D. Strausbaugh and Bernal R. Weimer. John Wiley and Sons, New York; Chapman and Hall, London. $3.75. 9 x 5¾; xi + 555 + 13 plates; 1938.

Animal Biology. *Second Edition.*
By Lorande L. Woodruff. The Macmillan Co., New York. $3.75. 8½ x 5¾; xiv + 535; 1938.

The first volume is a new college text which presents a wealth of biological material around the general foundation of the structure-function relationship. This aspect of the book should enable students who are being initiated into the wonders of biology to gain a fuller appreciation of the functioning organism as a whole.

The authors have made a successful attempt throughout the volume to proceed from the known to the related unknown, and from the general to the more specific. The sections on the animal and

plant kingdoms are developed around the natural history of the several forms, and give special emphasis to their economic value. Rather than setting aside a few pages for the stereotyped glossary, characteristic of so many scientific books, the authors have given the etymology and meaning of new terms as they are introduced to the reader. The text is well illustrated and indexed; the latter feature affording it extra value as a general reference.

The present (second—first edition noticed in Q. R. B., Vol. 7, No. 1) edition of Woodruff's well-known college text follows very closely the content and organization of the earlier edition. Slight changes are apparent, due to recent investigation, especially in those sections which have to do with genetics. A new chapter on the "Human Background" has been added, giving a brief account of the pre-historic races of man, and their cultural development.

JOURNEY TO MANAOS.

By Earl Parker Hanson. Reynal and Hitchcock, New York. $3.00. 8⅜ x 5½; 342; 1938.

Mr. Hanson was sent to collect observations on terrestrial magnetism in equatorial South America. His book is a most interesting and enjoyable story of his experiences along the way, and something more than just an adventure story. It is filled with many penetrating observations concerning politics, economics and social changes that are occurring in the regions that he covered. The biologist will find many interesting notes on plants and animals.

I pulled in a *caribe* one night, after I had lost three hooks and had finally shown the sense to use a steel leader. It was about six inches long, with a body like that of a bass, and with an undershot jaw like a bulldog's, its mouth filled with needle-sharp teeth. As it lay gasping I pushed into its mouth a hardwood stick as thick as my forefinger. The fish bit through it with one snap. Then I could believe all the stories I had heard of *caribes* stripping the flesh from unfortunate men in ten mniutes' time, thousands of them working together, each taking its own little bite.

Concerning the jungle he says:

The truth is that one cannot talk about the Amazonian jungle as one entity. It is not a single thing. It is a composite of thousands of jungles as alike and as different as the poles of the earth, scrambled together between numberless indefinable boundaries. There can be no such thing as one homogeneous jungle the size of the United States, and when two men argue about the nature of the Amazonian lowland forests they generally argue like blind men who have felt various parts of the elephant. Each generalizes from his own experiences in his own little part of that vast region of wild rivers and wilder forests.

BENEATH THE SURFACE. *The Cycle of River Life.*

By H. E. Towner Coston. Charles Scribner's Sons, New York; Country Life, London. $2.00. 7¼ x 5; 163 + 31 plates; 1938.

The author's many years' experience and observations in the field have made him exceptionally well qualified to write on the natural history of fresh-water life. Mr. Coston's love for nature, especially that phase of nature which has its existence beneath the surface, has not blinded him to the one fact which underlies much of his work; namely, that all activity among fish is derived from nervous reflexes and never from the process of thought.

The ecological factors influencing the cycles of life in rivers and streams throughout the year form the basis for discussion in this volume. The author presents much speculation on matters where scientific observation is difficult, but in such cases, he emphatically states that it *is* speculation and not scientific fact. The fascinating side of the study of life beneath the surface, in the words of the author, is that "we get a few facts, make a few scientific observations, then build up our knowledge on logical and ordered meditation."

The book is delightfully written, much of it in the first person, in such a manner as to make the reader feel that he has made the observations himself. The book boasts some 80 beautiful photographs, an appendix on "Cameras and nature photography," and an index.

THE ORGANIZATION OF NATURE PROTECTION IN THE VARIOUS COUNTRIES. *Special*

Publication of the American Committee for International Wild Life Protection No. 9.
By G. A. Brouwer. American Committee for International Wild Life Protection, Zoological Park, N. Y. 75 cents. $9\frac{1}{2}$ x 6; 112; 1938 (paper).

This is an English translation of the author's original Dutch publication of 1931. In the present book indices, lists and illustrations have been omitted, but an addenda of recent material is presented. The problems of geological, botanical and zoological preservation are discussed in the early pages, and then each continent is considered by countries as to methods for protection.

In Europe Germany maintains the most extensive organization for nature protection. The British are very efficient in preserving larger animal species in their African colonies, while Japan is active among the Asiatic countries. The United States and Canada keep pace with each other in protection, both being very efficient. There are a number of international congresses and organizations, most of which are devoted to some specific type of protection. A list of these is included. The one bureau which does not limit itself in this way and which deserves greater renown is the "Office international de documentation et de corrélation pour la Protection de la Nature."

THE ORIGIN OF LIFE.
By A. I. Oparin. Translation and Annotations by Sergius Morgulis. Macmillan Co., New York. \$2.75. $8\frac{1}{2}$ x $5\frac{1}{2}$; viii + 270; 1938.

The primary assumptions are that life originated on this earth and that it neither arose spontaneously nor existed eternally, and consequently must have resulted from the evolution of matter. Starting with simple hydrocarbons, the author builds up to high molecular compounds, and as he progresses towards life by the gradual evolution of organic substances, he illustrates his assumptions by physico-chemical reactions that are known to occur. Of course there are many gaps, but the author is optimistic. "The road ahead is hard and long but without doubt it leads to the ultimate knowledge of the nature of life."

NATURE PHOTOGRAPHY AROUND THE YEAR.
By Percy A. Morris. D. Appleton-Century Co., New York. \$4.00. $8\frac{1}{2}$ x $5\frac{3}{4}$; xviii + 251 + 96 plates; 1938.

Each month nature reveals something new to be photographed, whether in June or December, and with each new subject a different technique has to be applied to obtain a successful photograph.

The author has hit upon a pleasant way of mixing his nature and photography. For each month he tells which plants and animals are available, what they are doing, and then how best to proceed in order to get a successful "shot." The nature lover will find his interest in photography aroused as will the photographer his interest in nature.

There are numerous illustrations some of which, unfortunately, are rather inferior.

BIOCLIMATICS. *A Science of Life and Climate Relations. U. S. Department of Agriculture. Miscellaneous Publication No. 280.*
By Andrew D. Hopkins. Government Printing Office, Washington. 35 cents. $11\frac{1}{2}$ x $9\frac{1}{4}$ inches; iv + 188 + 1 folding chart; 1938 (paper).

This is a very comprehensive study of bioclimatics, particularly with regard to agriculture. The researches have been based on a life-time effort of the author, who has had more than a half-century of practical experience in agriculture and more than 40 years as an official entomologist. It would be surely worth while to apply the methods worked out in this study to some special problems in epidemiology and human biology.

HUMAN BIOLOGY

ON THE PROBLEMS CREATED BY THE PREMATURE SUBDIVISION OF URBAN LANDS IN SELECTED METROPOLITAN DISTRICTS IN THE STATE OF NEW YORK. *A Report to the State Planning Council of New York.*
By Philip H. Cornick. Division of State Planning, Albany. \$1.00. 9 x $5\frac{3}{4}$; xxi +346; 1938 (paper).

We have all, no doubt, seen on the out-

skirts of cities real estate developments that had failed to develop and have pitied the poor suckers who had lost their money by buying lots. The thesis of this book is that the loss is spread over a much larger group than the buyers of lots. The promoter of the development usually buys the land on mortgage from the original owner and induces the local government to install water mains, sewers and street paving. The money for his sales campaign he borrows from a bank. If the rate of subdivision is no greater than the effective demand for lots, well and good. With the money which the promoter receives from the sales of lots he can pay off his mortgage to the original owner and his note to the bank and still have a profit. But promoters are a sanguine tribe. Where one has made money others hope to be equally fortunate, so that in time the supply of lots exceeds the demand. As the boom begins to ebb and prices of real estate to decline the promoter finds it harder and harder to sell his lots. The time soon comes when his indebtedness to the original owner and to the bank and the arrears on his taxes are greater than the current value of the unsold lots. He has lost what money he had put into the development. The same is true of many of the purchasers on installment contracts. Even the arrears of taxes are often greater than the current value of the land, so that neither the original owner nor the bank finds it profitable to foreclose. Both of them have lost money. Finally the high cost of foreclosure on each lot debars the municipality from taking over the land, which stands idle, while the cost of the unused improvements is borne by the other property owners of the city. Everyone loses.

How can this vicious process be prevented? In Cincinnati developers of land are required to install all necessary improvements at their own expense before the city will accept the dedication of the streets. If the developer should seek to evade such a provision by permitting the streets to remain private, the state or municipality, under its power to protect the health and safety of its inhabitants, could require, before a building permit could be issued, that proper provision be made for the supply of water and the disposal of sewage. It could, moreover, require that all deeds should state that the property fronts on a private street and that the municipality has no right or obligation to install improvements on such a street. Such measures

> would take the joy out of subdividing cow pastures by saddling the costs and liabilities now borne by the general public on those who engage in such activities for profit. They would therefore impose a much needed economic check on the volume of such activities, and would tend to hold the prices—and therefore the taxable valuations—of the cow pastures down to a level at which their profitable use as cow pastures might continue.

THE STUDENT AND HIS KNOWLEDGE. *A Report to the Carnegie Foundation on the Results of the High School and College Examinations of 1928, 1930, and 1932. Bulletin Number Twenty-nine. Study of the Relations of Secondary and Higher Education in Pennsylvania.*

By William S. Learned and Ben D. Wood. With a Foreword by Walter A. Jessup. Carnegie Foundation for the Advancement of Teaching, New York. Free. 10 x 7½; xx + 406; 1938 (paper).

This study is an inventory of the knowledge of college students. In 1928 a comprehensive examination covering mathematics, the physical and natural sciences, the social sciences, literature and the fine arts was given to approximately 4,500 senior students in Pennsylvania colleges and approximately 27,000 high-school seniors. A revised test along the same lines was applied in 1930 to about 6,300 college sophomores and in 1932 to about 3,700 college seniors. For about 1,200 students results of the examination taken as high-school seniors, as college sophomores and as college seniors are available.

The most striking result is the wide range of knowledge shown by students of the same school status. In 1928 the college seniors showed a range of scores from 110 to 1580. Their coefficient of variation was 33 per cent. Yet all of these students, having spent four years at college and obtained the required number of credits, were to receive from the college the official stamp of educated men.

Although the average score of the college seniors is much higher than that of the sophomores, 28 per cent of the seniors do less well than the average sophomore and nearly 10 per cent do less well than the average high-school senior. So also the average high-school senior score is below the average college sophomore level but 22 per cent of this secondary-school group surpass it and 10 per cent exceed the college-senior average. In other words, students classified as sophomores range, as to their command of knowledge appropriate to their status, from a general level of inferior high-school achievement to one attained only by the best 10 per cent of senior college students—indeed, above the average of faculty groups.

A disquieting result is the low rank of students of education. The median score of such students in arts colleges was well below the general median and the median score of students in teachers colleges was still lower. In these colleges the median for sophomore students of education was actually below that of the high-school seniors. The prospective teacher knew less than the students he (or she) was expected to teach.

Transactions of the American Philosophical Society Held at Philadelphia. *New Series*, Volume XXIX, Part II. Article II, *The Variation in the Silicate Content of the Water in Monterey Bay, California, During 1932, 1933 and 1934*, by Austin Phelps; Article III, *The Old Stone Age in European Russia*, by Eugene A. Golomshtok.

University of Pennsylvania Press, Philadelphia. $3.00. 11¾ x 9¼; 153–468 + 37 plates; 1938 (paper).

This issue of the Transactions of the American Philosophical Society consists of two articles, the first of which is a discussion of the silicate content of Monterey Bay by Austin Phelps, in which the silicate concentration is shown to be a function of the depth and temperature. The second article deals with palaeolithic man in European Russia, and is by Eugene A. Golomshtok.

Russia occupies a critical position in anthropology and archaeology, because while it is pretty generally accepted that man originated in south east Asia, practically all the information we have about palaeolithic man has come from finds in western Europe, and it is difficult to see how he could have arrived there without crossing Russia. Yet until recently no one has thought seriously of searching that country for fossils or artifacts.

The greater part of the present work is a highly technical discussion of material from fifty-five different sites in European Russia, with excellent photographic and hand-made illustrations. But the interpretation of these finds is attended by a great deal of difficulty, as there is wide divergence of opinion among Russian authorities as to the correlation of the horizons in which they occurred with the type horizons in France. The latter are cultural horizons and represent successive stages in development; the two species and various subspecies of man may have passed through corresponding stages at different periods, so that contemporary cultures in separated geographic localities are not necessarily homologous; indeed the contrary is generally the case. The two most primitive cultural horizons in France, the Chellean and the Acheulean, have not as yet been detected in Russia (with the exception of one find of doubtful authenticity) and even geologists disagree as to how many glaciations there have been in that country, and how they correlate with the four advances of the ice that have been recognized in Western Europe and North America.

The author does not draw any conclusions of his own, but states those of most of his contemporaries, discussing with all fairness, both the facts that seem to support them and those that militate against them. The bibliography covers seven pages and the index eight.

The Apache Indians.

By Frank C. Lockwood. The Macmillan Company, New York. $3.50. 8½ x 5⅝; xvi + 348 + 42 plates; 1938.

In essence this book is a well-written account of the struggle between the Apache Indians and the United States government, with the latter, sad to note,

not shining very brightly either in the manner of conducting the conquest or in that of abiding by its treaties. With deep sympathy for the Apache, the author briefly outlines what is known about the origin of these peoples, their culture, social system and their savagery which was to engender a healthy fear in both the Spanish and the Mexicans. The earliest recorded account of a clash between the Apache and Americans took place in 1825 but a friendly understanding was reached and maintained for 10 years. In 1835 hostilities recommenced, the whites being apparently at fault, and continued off and on until 1886. At this date the last group of Apache, 36 in all, including 19 women and children, voluntarily gave themselves up to 5000 soldiers who had been after them for over 4 months. As it had before, the government again broke its promises and instead of allowing the Apache to return to the original reservations given them, imprisoned them all, friendly and hostile Indians alike, for four years. In 1890 the survivors were finally allowed to return to the reservations where their descendants now live. To a varying degree the Apache have adapted themselves to the white man's way of living. They have apparently not suffered from the contact with the white man, since in 1936 they numbered about 7500, which is nearly 1500 more than in 1886. In general, the author finds them industrious and very able in certain types of work, but in moral matters, according to Christian views, they have progressed very little and in this respect the author holds little hope for the future. This book, besides its literary and historical value, presents a number of interesting features for the human biologist. Not the least is the contrast between that period in the nation's history and the immediate present characterized by loud proclamations regarding the sanctity of treaties and the official verbal excoriation of aggressor nations.

Isaac Newton, 1642–1727.
By J. W. N. Sullivan. With a Memoir of the Author by Charles Singer. The Macmillan Co., New York. $2.50. $8\frac{1}{4} \times 5\frac{1}{4}$; xx + 275; 1938.

This book is, we regret to say, the last work of one of the ablest interpreters of science to the public. In the introductory memoir of Sullivan Singer writes: "In describing his mental characteristics the phrases that seem to me best to recall his special qualities are 'intellectual integrity' and 'capacity to penetrate to the essential nature of a problem.' "

In his prefatory note Sullivan writes: "I have long been impressed by the fact that Isaac Newton, besides being the greatest of scientific geniuses, was also one of the most singular and fascinating characters of which we have any record." The puzzling question to which Sullivan directs his analysis is this: Why did Newton let his unique combination of gifts as a mathematician and as an experimentalist lie fallow for so much of his life? After the period in his twenties in which he made his fundamental discoveries on the composition and refraction of light and laid at least the foundations of the differential calculus and the theory of gravitation Newton's interest in scientific work steadily declined until it was rekindled by Halley. And after the seventeen months of intensive work that produced the *Principia* he spent the remaining forty years of his life in work on theology and chronology and in his official duties as Master of the Mint with only occasional attention to scientific problems. Why was this? Sullivan's answer is that Newton felt that science was relatively unimportant. "Newton genuinely believed that man was part of a Divine Scheme, and that the material universe was no more than the setting within which part of his eternal destiny was to be worked out. . . . The paradox of Newton's scientific career is due to the fact, probably unique in the history of scientific men, that he was a genius of the first order at something he did not consider to be of the first importance."

Civilization and Disease.
By C. P. Donnison. With an Introduction by Sir Walter Langdon-Brown. William

Wood and Co., Baltimore. $3.00. 8½ x 5¼; xv + 222; 1938.

Taking as a point of departure the stated fact that among the peoples of the Western civilization, when compared to other ethnic groups, there is a higher incidence of hyperpeisia, Grave's disease, peptic ulcer and diabetes, the author discusses the probable factors related to this difference. He summarizes the information about the possible etiological causes and is led to believe that a psychogenic factor associated with civilization is responsible for the relative prevalence of the above diseases among the white peoples. The greater part of the book is taken up with an inquiry regarding this factor. The author examines critically the theories of Freud and his school, of McDougall and others and finally concludes that the psychic factor arises from a conflict due to the lack of proper social integration. In other words, while the savage from the earliest period of his life is in contact with the remainder of his social group, the civilized youngster is not and must readjust himself after his behavior has already been conditioned by the selected small group which surrounds him. An unsatisfactory readjustment will therefore lead to a psychic imbalance. There is much to be said for such a theory which leans strongly towards Adler's ideas. It deserves consideration even though, as the author notes, the data purported to show the differences in disease incidence between the savage and the civilized are far from satisfactory, and much has yet to be learned about psychic factors in the causation of organic diseases.

La Population de la France. *Son Évolution et ses Perspectives.*

By Michel Huber, Henri Bunle and Fernand Boverat. Preface by A. Landry. Librairie Hachette, Paris. 30 francs. 8½ x 5½; xiii + 250; 1937 (paper).

The first two parts of this book, ably prepared by Huber and Bunle, constitute a complete summary of the demographic conditions of France at present and in the past, and include comparable data regarding other European countries, particularly Germany. The facts presented are well known: (1) the French population is practically stationary and most of the recent slight increase is due to immigration, (2) the birth and death rates continue to decrease but the latter is still relatively high. The prospects for the future of the French population are therefore exceedingly dark according to Boverat who discusses these observations. In contrast to some students who believe that a stationary or declining population is a blessing, Boverat belongs to that school that sees in such a trend the downfall of a nation, because industry and agriculture are adversely affected and due to their interdependence both will eventually fail. To remedy this situation which seems to him very serious he calls, as is usual, for government aid. First of all, he desires more stringent laws against abortions. Secondly he proposes a campaign of education in the schools so that children will learn early that large families are needed. An appeal is to be made to their patriotism and if this fails monetary incentives should be tried. In view of the marked lack of success which has followed such plans in the past in France as well as in other countries, it is doubtful whether this one, should it be followed, will alter appreciably the situation.

The Human Organism and the World of Life. *A Survey in Biological Science.*

By Clarence W. Young, G. Ledyard Stebbins, and Clarence J. Hylander. Harper and Brothers, New York. $3.00. 8½ x 5½; viii + 657; 1938.

This work differs from most other biological treatises chiefly in its method of approach. Instead of contemplating the science from the aesthetic standpoint of the seeker who pursues truth for its own sake without regard to where it may lead him, the authors have chosen to approach from the cultural or practical standpoint. They are not interested in inspiring the student to undertake original work so much as to awaken in him an appreciation of what others are doing, that he may understand how those in contact with him react to and interpret their common en-

vironment. Further, the field of biological research includes man himself. The Socratic philosophy was based upon the dictum "Know thyself," and a greater teacher than Socrates is reported to have said "The kingdom of God is within you, and he that knoweth himself shall find it." It is no longer fashionable to speak of the body as the environment of the soul, but the fact remains that man is a conscious being in possession of a physical body from which he cannot be separated during his life, and it is therefore obvious that a knowledge of how the body came to be, how it functions, and how it should be cared for is an essential component of a liberal education. The authors have produced a readily readable book covering these matters which is addressed not to the specialist but to the average reader, and which deserves wide recognition. The sections dealing with endocrinology and psychiatry are especially timely. The index covers 18 pages.

Russian Medicine. *Clio Medica.*
By W. Horsley Gantt. Paul B. Hoeber, Inc., New York. $2.50. $6\frac{1}{2} \times 4\frac{1}{2}$; xiii + 214; 1937.

In an interesting and scholarly manner Gantt traces the trend of medical practice and teaching in Russia from its very early primitive form to the much publicized present day system. Western physicians were first introduced in Russia about the beginning of the 15th century but, as for all arts and sciences, it was not until Peter the Great, himself a student of medicine, began the attempt to reorganize Russian society along Western lines that hospitals were established and trained physicians began to replace faith-healers, bone-setters and other unorthodox practitioners of the art of medicine. By the end of the 19th century, although the standards of medical training in Russia were inferior to those of the occidental world, already biologists such as Von Baer had been produced, and surgery had its Pirogov, physiology its Sechenov, Mechnikov and of course Pavlov. In addition, the first serious attempt towards the creation of a socialized system of medicine had already been made with the organization, *ca.* 1870, of the Zemstevo, the direct ancestor of the Soviet system. Regarding the latter, Gantt has a great deal of praise not unmixed with skepticism. He has personally observed the immense progress in public health made during the Soviet regime, but like all unbiassed observers he questions whether this progress is due to communism *per se.* The book is well indexed, and contains a short but adequate bibliography and a table of comparative chronology of medical events in Russia and in the remaining world.

Les Crises de la Morale et de la Moralité *dans l'Histoire de la Civilization et de la Littérature des Pays Anglo-Saxons.*
By Paul Yvon. Boivin et Cie, Paris. 20 francs. $9 \times 5\frac{1}{2}$; 127; 1937 (paper).

The French, Yvon notes, picture the typical Englishman as a strait-laced sourpuss. This view is apparently justified by the almost universal tendency on the part of English as well as American writers to preach a sermon on morals. Since literature, to a certain degree, mirrors the mores of a given country and period, the author investigates the mode of development of this moralistic tendency in the English literature of 1660 to 1820 and describes the social factors associated with it. He distinguishes three periods: 1660–1710, 1710–1760 and 1760–1820. The first period, characterized by the writings of Wycherley, Behn and Dryden, manifests clearly the overt libertinage of the aristocrats of the Restoration. In the second period, the Stuarts having been superseded and the middle class having attained a share of the power in government, the literature is realistic but the moral conventions are no longer so openly flouted. In this period we find Addison, Steele and Sterne. Between 1760 and 1820, with the rise in power of the middle class and the conservative reaction to the French revolution, the tendency to moralize becomes more marked and is finally transformed into the Victorian rigorism and artificiality. As conducted here, this analysis is rather superficial and not very informative. It does, however, demonstrate the

benefits of utilizing another source, literature, in the study of social phenomena.

EARLY SCIENCE IN OXFORD. *Vol. XI. Oxford Colleges and Their Men of Science.*

By R. T. Gunther. R. T. Gunther, Old Ashmolean Building, Oxford. £1.10. $8\frac{1}{4}$ x $5\frac{1}{4}$; xvi + 429 + folding chart; 1937.

The latest volume of Dr. Gunther's series gives brief biographic notes on Oxford scientists, arranged by colleges. These range in time from John of Gaddesden, of Merton College, the author of the *Rosa medicinae* who graduated M.D. about 1309, to the twentieth century. Among the names of greatest interest to a biologist are Harvey, Baillie, Cunningham, Lyell, Ray Lankester, Gilbert White, Pennant, Bather, G. S. Haldane, Linacre, Sydenham, Wren, Mayow, Burdon-Sanderson, Willis, Lower, Hooke, Osler, Highmore, Wharton, Poulton, Tyson and Rolleston. "In 1845, Dr. Gunther writes, "the attitude of Oxford to science was not so much hostile as contemptuous. It is true that the existence of science was asserted by thirteen salaried professors, a Museum, A Physick Garden, and an Anatomy School; but the professors did not lecture: the Museum contained little beyond a verminous giraffe, a lode-stone, a mummy, King Alfred's jewel, and a fine twelfth-century Bestiary which was shown to visitors who could pay sixpence." If Oxford is now more hospitable to science, there is evidence in this book that the history of science is still a Little Orphan Annie.

THE HYGIENE OF HOUSING. *Report by the Housing Commission. League of Nations, Bulletin of the Health Organization, Vol. IV, No. 4, August 1937.*

Edited by the Health Section of the League of Nations, Geneva. Publications Department of the League of Nations, Geneva; Columbia University Press, New York. 65 cents. $9\frac{1}{2}$ x 6; 505–682; 1937 (paper).

This number of the Bulletin contains two articles. The first is a report by the Housing Commission on the subject of the hygiene of housing. In this report, a first part discusses the scope and organization of the commission; in the second part are presented the results of a very general survey of expert opinion on the methods of heating and cooling houses in France, England and the United States. The report concludes with a plea for more data and international cooperation.

The second article is a review by Wroczynski of studies on physical development and physical defects in children. The author considers, in particular, certain methods of physical measurements, the physiologic changes accompanying exercise and the types of physical education used in several countries. The main conclusion reached is that notwithstanding the importance given at present to physical education the prevalence of physical defects has not been reduced. This study is characterized by superficiality and an inadequate bibliography, especially that referring to investigations made in this country.

CLAUDE BERNARD *Physiologist.*

By J. M. D. Olmsted. Harper and Brothers, New York and London. $4.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; xvi + 272; 1938.

It is unexplicable why so few biographies of Claude Bernard have been published; this is the first English book-length study which has appeared since 1899. Yet not only were Bernard's achievements remarkable but his whole philosophical attitude towards research was unique and deserves to be meditated upon by all students of science. In addition his personal life was not devoid of human interest. He came of peasant stock, received a somewhat mediocre secondary education and entered the Paris medical school at the age of twenty-one. For a year and a half before that he had been a pharmacist's apprentice and had unsuccessfully attempted to become a playwright. Even after he entered medical school Claude Bernard found himself only after he came under the influence of Magendie. The latter impressed his views and methods on the brilliant assistant and aided his advancement. The details

of Bernard's career, the significance of his work, his marital unhappiness are all fully given in this book. Written with admiration and understanding of the man and the scientist this biography is a real contribution to the history of biology.

ARCHITECTS OF IDEAS. *The Story of the Great Theories of Mankind.*

By Ernest R. Trattner. Carrick and Evans, New York. $3.75. $9\frac{1}{4} \times 6\frac{3}{8}$; [8] + 426 + 15 portraits; 1938.

In chronological order, beginning with Copernicus and ending with Einstein the author outlines the biography and scientific contributions of 15 men whose ideas are considered to be the foundation of modern science. A chapter is dedicated to each of them and each chapter includes a brief discussion of the predecessors and followers of the principal character as well as a consideration of the import and consequences of the theory he advanced. The "architects" include Copernicus, Hutton, Dalton, Lavoisier, Rumford, Huygens, Malthus, Schwann, Darwin, Marx, Pasteur, Freud, Chamberlin, Boas and Einstein. Naturally, any such list is always open to criticism and this one seems particularly vulnerable. Aside from this, the book is a satisfactory source of information about some of the main developments in the history of science. It is written in a pleasing style bordering on the journalistic. A few times the lightness of touch and economy of language is carried too far and the following results: "For many years she (Pasteur's wife) was to bear him children and hold his meals hot while he stayed overtime in his laboratory."

URBAN SOCIOLOGY.

By Earl E. Muntz. The Macmillan Co., New York. $3.75. $9\frac{1}{4} \times 6\frac{1}{4}$; xvi + 742; 1938.

This text attempts to cover, and does so rather successfully, the various aspects of urban development in the United States. In the first part there is a brief historical account of urbanism and chapters on the growth of modern cities especially those of the United States, problems of rural and foreign migration, and city planning. The second part concerns housing, its progress in recent years and the development of suburban areas. In the third part the author discusses public health, sanitation and safety as achieved in urban areas: the problem of water supply, garbage, hospitals and medical and school care. Education is the topic of the fourth part which describes the development of public, vocational and special schools. The fifth part deals with recreation, public and private amusement centers. The presentation of the subject is historico-descriptive and factual without any attempt to theorize on the past or to predict the future. Interesting data are included and there is an adequate bibliography.

SINGING FOR POWER. *The Song Magic of the Papago Indians of Southern Arizona.*

By Ruth M. Underhill. University of California Press, Berkeley. $2.00. $8\frac{1}{2} \times 5\frac{1}{2}$; vii + 158; 1938.

The author spent 14 months in the Papago country of southwestern Arizona studying the everyday life and activities of this quiet race, and in this little volume she has recorded much of their folk-lore and customs, as well as the translation of many of their magic songs. The underlying foundation of the Papago Indian life, and the source from which they derive their deepest thought and power is the magic of song. Whether they be working, playing, worshipping, harvesting, or making war, these Indians find expression for their innermost feelings in the form of song.

The book is interestingly written in a style (first person and present tense) which makes delightful reading for both those keenly interested in Indian folk-lore, and those interested in the varied and subtle forms of human nature.

CRIME, CROOKS AND COPS.

By August Vollmer and Alfred E. Parker. Funk and Wagnalls Co., New York. 2.00. $7\frac{1}{2} \times 5$; v + 260; 1937.

The authors, in writing this little book,

have made an exceptional appeal to two groups of people: (1) those who delight in mystery and crime detection stories, and (2) those who are convinced that anything but justice comes from our present court and prison systems.

The first part of the book deals with numerous "baffling cases" in the history of crime detection, and gives some idea of how science is being used and must be more and more used in solving crimes which are planned by master minds, and which approach perfection. The latter portion of the volume deals strongly and bluntly with the inefficiency of our court systems; with the incongruity of mixing politics with law and expecting to get justice; and with the deplorable status of our prisons and reform schools. Throughout the volume there is an appeal to incorporate into our social system better trained police officers, attorneys, judges, and prison personnel.

Die Akklimatisation. *Eine Untersuchung über ihre Bedingungen, ihre Fehlschläge und ihre erfolgreiche Führung.*

By J. Grober. Gustav Fischer, Jena. RM. 6.50 (paper); RM. 8.00 (bound). $10\frac{1}{4} \times 6\frac{3}{4}$; 156; 1936.

Although written by a professor of medicine this book is not intended for medical men alone, but also for merchants, political economists, soldiers, and especially for statesmen. Its purpose is to serve the expansion of nations in the settlement of the earth's area and the welfare of the settlers. To this end the author has emphasized the mistakes made in the colonization programs of the past. Material on the introduction of new crops and animals into a region and the conversion of waste-land into profitable areas is discussed to some extent but the main portion of the book is devoted to the acclimatization of man, especially of the white race, to various environments. The effects of changes of climate are discussed not only relative to the generation that has migrated either as adults or in childhood, but also in respect of the children born in the adopted clime. The treatment of the subject is general but the book should nevertheless be useful to anyone interested in population problems. There is a short list of authors cited and an index.

Africa's God. *VIII. Rhodesia. Anthropological Series of the Boston College Graduate School, Vol. III, No. 1.*

By Joseph J. Williams, S.J. Boston College Press, Chestnut Hill, Mass. $1.00. $9\frac{3}{4} \times 6\frac{3}{4}$; 37; 1938 (paper).

The eighth report on the religion of Africa is concerned with Rhodesia, where, as in other parts of Africa previously reviewed, monotheism prevails. Although some tribes, such as the ba-Tonga, are found to be strictly monotheistic, others show but obscure and vestigial tendencies towards monotheism. The question of possible diffusion, especially Hebraic, or of independent origin of the monotheistic culture is considered, as in former reports. In addition to interesting, but highly speculative, theories on linguistic derivations, various tribal laws, customs, proverbs, and legends are included.

In view of the irreparable losses engendered by past faulty records in archaeological field work, there is an additional article by J. W. Murphy, *S.J.*, who presents a brief survey of the careful procedure of prehistoric excavations at Ksar 'Akil.

Yukon Voyage. *Unofficial Log of the Steamer Yukoner.*

By Walter R. Curtin. Caxton Printers, Caldwell, Idaho. $3.50. 9×6; 299 + 56 plates; 1938.

For lively and unadulterated adventure, "Yukon Voyage" is hard to beat. The volume presents in dramatic fashion, the unofficial log of the steamer "Yukoner" on her gold rush voyage from St. Michael to Dawson in the years 1898–99. For many of the individuals aboard the marooned ship, the eight months spent in almost complete isolation seemed a dreary and unbearable eternity, but for Walter Curtin it was, in his own words, "the most enjoyable vacation I ever had." Mr. Curtin's observations and his reactions to his surroundings were clearly set

down in an orderly, yet subtly humorous fashion in his diary. The majority of this work is taken directly from the pages of the diary. The work is well illustrated throughout by drawings and photographs. As a result of the interest in human nature, and the display of pure adventure, the book will undoubtedly be a source of absorbing enjoyment to many of its readers.

Middle Age is What You Make It.

By Boris Sokoloff. The Greystone Press, New York. $1.75. $7\frac{3}{4}$ x $5\frac{1}{2}$; xv + 204; 1938.

The author of this interesting little volume was a student of Metchnikoff, and in consequence he incorporates into his text much of the Metchnikoff school of thought. The book is not intended so much for the specialist of the medical profession, or even the general scientist, as it is for the man of the street. It is written in a popular style, and contains much encouragement for the man of forty who is convinced he is growing old. Dr. Sokoloff emphasizes the need for the layman to learn something of his own normal physiological processes, and to be guided by a competent physician in his search for health and happiness during middle age, instead of by the high pressure patent medicine advertisements.

Baron Constantin von Economo. *His Life and Work.*

By his wife and by J. von Wagner-Jauregg. Translated from the second German edition by Ramsay Spillman. (Obtainable from the Translator, 115 East 61 St., New York). $2.00. $9\frac{1}{2}$ x $6\frac{3}{8}$; x + 126; 1937.

This is a fascinating biography of the famous investigator of cerebral histology and discoverer of encephalitis lethargica, who was a pioneer aviator, flying his own plane as early as 1908. The biography of Economo, who died in 1931 at the age of 55, is written by his devoted wife. The psychiatrist von Wagner-Jauregg, Vienna clinician and teacher of Economo, adds a brief survey of his scientific work. The English text is further augmented by the three papers which Economo read in New York in 1929—Sleep as a problem of localization; Some new methods for studying brains of exceptional people; Cytoarchitectony and progressive cerebration. The translator is right in regarding this biography as a great human document.

A Handbook of Methods for the Study of Adolescent Children. *Monographs of the Society for Research in Child Development. Volume III, No. 2 (Serial No. 15).*

By William W. Greulich, Harry G. Day, Sander E. Lachman, John B. Wolfe, and Frank K. Shuttleworth. Society for Research in Child Development, National Research Council, Washington, D. C. $2.25. 9 x 6; xvii + 406; 1938 (paper).

This monograph was prepared as a methodological aid for the study of child development. The material is presented in five parts: I. Some anatomical aspects, II. Some biochemical and physiological aspects, III. Respiration and energy metabolism, IV. Some psychological aspects, V. Problems not involving direct measurements of children. The subject matter of each part consists of concise formulations of methods available for investigating problems relating to child development. Essential known facts with regard to each problem are stated briefly, and the basic literature is listed at the conclusion of each separate discussion. A tremendous mass of material is made readily available through this handbook which should be invaluable to investigators in all fields of child development.

An Introduction to Physical Anthropology.

By E. P. Stibbe. With an Appendix by W. A. M. Smart. Edward Arnold and Co., London; Longmans, Green and Co., New York. $3.25. $8\frac{5}{8}$ x $5\frac{1}{4}$; vii + 230; 1938.

This edition (first reviewed in Q. R. B. Vol. 6, p. 361) shows considerable revision. A. H. Munter has assisted in some of the text work, Le Gros Clark has been consulted on a number of points concerning the primates, and W. A. M. Smart

contributes a short appendix on the statistical examination of anthropometric data. The general treatment of the material is simple and clear enough to give an excellent introduction into methods and problems of physical anthropology. Figures, a glossary of technical terms, and a well-arranged index are included in the volume.

FEARFULLY AND WONDERFULLY MADE. *The Human Organism in the Light of Modern Science.*

By Renée von Eulenburg-Wiener. The Macmillan Co., New York. $3.50. $9\frac{1}{4} \times 6\frac{1}{4}$; xii + 472; 1938.

The book deals with the functions of human organism, its physiology, anatomy and behavior as a whole. Its point of view is philosophical as well as biochemical and bio-physical. The author has found it necessary to enlarge upon the physico-chemical laws and especially to inquire into the significance of the asymmetric structure of the bio-molecules for the phenomena of life. The great Pasteur recognized that this asymmetry was the one distinct line of demarcation between the organic and inorganic world, as the writer notes in her thoughtful introduction.

THE BANTU TRIBES OF SOUTH AFRICA. *Reproductions of Photographic Studies. Vol. III, Section III, Plates LXXXI–CXX. The Nguni. Section III, The Zulu. With an Introductory Article on the Zulu, a Bibliography, and Descriptive Notes on the Plates by D. McK. Malcolm.*

By A. M. Duggan-Cronin. Deighton, Bell and Co., Cambridge. 25s. net. $11\frac{3}{4} \times 8\frac{1}{4}$; [8] + 16 + 40 plates; 1938 (paper).

This volume, which represents a small portion of the entire study of the Bantu tribes of South Africa, concerns itself with the Zulu only. A brief note dealing with the geneology and social customs of the tribe is presented, together with an extensive bibliography of studies of South African tribes in general. The greater portion of the volume is devoted to pictorial studies of the dress, homes, customs, activities and environment of the Zulu.

ZOÖLOGY

BIRD FLOCKS AND THE BREEDING CYCLE. *A Contribution to the Study of Avian Sociality.*

By F. Fraser Darling. The University Press, Cambridge; The Macmillan Co., New York. $1.75. $7\frac{1}{2} \times 5\frac{1}{4}$; x + 124; 1938.

The author, with his wife, spent almost two years on Priest Island, a small uninhabited island off the North West Highlands, observing the social behavior of various species of sea-birds. From blinds in different parts of the island he could make almost continuous observations on five colonies of gulls through their entire breeding cycles. From these studies, he concluded that successful breeding, mating, and care of the young depends largely on communal life.

The book contains a summary of recent discoveries in the field of endocrinology which, he believes, might help to explain the cyclical behavior of these flocks of birds. His interest lies particularly in glandular alterations produced by external stimuli such as changes in light, temperature, and social contacts.

Fraser Darling appears to have something less than wholly adequate power to present his own highly important results in the most effective way. The distinction of his work lies in its *insight*, unsurpassed in the domain of behavior research under natural, field conditions. This brief notice may well conclude with an illuminating quotation.

> The brain of the bird does not have the cerebral cortex which distinguishes the mammalian brain. If conjecture were to be made on the possible further evolution of avian behaviour, it would probably be suggested that the bird is moving along the line of perfection of instinctive behaviour rather than following that of an increasing tendency towards grasping new situations and wholes, and making ready adaptations which are characteristic of the development of mammalian behaviour. Do not let me be thought to say that the bird we know is incapable of adaptive behaviour, for it would be easy to point to many remarkable examples; but what appears to us as a very complicated pattern may be found to be explicable on a few key reactions.

When considering the manifold effects of the mate or companion on the life of the individual bird, it is well to remember how improbable it is that the individual can have any clear realization of self. It cannot say to itself, *Cogito, ergo sum*. And I think this lack may impose the necessity of a companion or mate to supply visual and auditory stimulation in order to live more or less fully. This need, expressed in many ways, may be supplied by the one mate or more than one in birds of different social thresholds. One action calls forth another from the mate or companion and that in turn may lead the first bird to a third pattern. A chain of distinctive behaviour is elicited by this reciprocation.

THE LIFE STORY OF THE FISH.

By Brian Curtis. Introduction by William Beebe. D. Appleton-Century Co., New York and London. $3.00. $8\frac{1}{8}$ x $5\frac{5}{8}$; xiv + 260; 1938.

There is nothing dry about this fish story. The author succeeds amazingly well in achieving his expressed intention of giving "a general understanding of why fish behave as they do." A fund of scientifically accurate information on fossil as well as recent fish, is given by Mr. Curtis in a most lively and entertaining style. His sense of humor is as keen as his power of observation and he has consummate skill in transmitting his knowledge and eager enthusiasm to his readers.

Even if heretofore the reader's knowledge of a fish has been confined to specimens caught with a can-opener, after reading this book, interest in the whole fish, its structure, characteristics, behavior and habitats will be increased a hundred fold. As William Beebe aptly says in his introduction to the book, "as you read it, you learn and laugh, and learn again." The physiological relationship between fish and man is most engagingly set forth, and Mr. Curtis answers many questions that pertain to the entire field of biology. The amateur fish-fancier with an aquarium in the window, the fisherman, and sportsman who wants to know more about fish in general, or game fish in particular, as well as the student of ichthyology or biology will each find just the information he desires.

Excellent diagrams, plates and charts fully illustrate the text, and there is an adequate index of six pages.

INDEX-CATALOGUE OF MEDICAL AND VETERINARY ZOÖLOGY. *Part 1. Authors: AALL to AZZOLINA. U. S. Department of Agriculture.*

By Albert Hassall and Margie Potter. Government Printing Office, Washington. 10 cents. $\frac{1}{4}$ x 6; 142; 1932 (paper).

INDEX-CATALOGUE OF MEDICAL AND VETERINARY ZOÖLOGY. *Part 2. Authors: B to BYCHKOV. U. S. Department of Agriculture.*

By Albert Hassall, Margie Potter, Mildred A. Doss, Marion M. Farr and Gertrude B. Carson. Government Printing Office, Washington. 55 cents. $9\frac{1}{4}$ x 6; 143–612; 1937 (paper).

The U. S. Department of Agriculture is now issuing a revised and enlarged edition of the *Index-Catalogue of Medical and Veterinary Zoölogy—Authors* (first published in 1902–1912). This is a most valuable addition to any biological library and since it will be some time before all the parts are out it behooves those who possess the early numbers to see that a special place is reserved for the addition of future numbers and to obtain them. The various parts of the catalogue are no longer being published under the joint authorship of Stiles, Hassall, *et al.* Dr. Stiles is continuing the publication of the host catalogue from the National Institute of Health, and Hassall, Potter *et al.* have taken over the separate publication of the author catalogue. Each part of the author catalogue has sections on abbreviations for single words, for libraries, and a key to serial abbreviations, all of which precede the index section.

ОБШАЯ ЗООГЕОГРАФИЯ

В. Г. Гептнер. Государственное Издательство Биолотической и Медицинокой Литературы. Москва-Ленинград. 548; 13 рубл. 60 коп.; 1936.

[GENERAL ZOÖGEOGRAPHY.

By V. G. Heptner. State Publishing House for Biological and Medical Literature, Moscow and Leningrad. 13.60 roubles. 548; 1936.]

This important book is written by a zoölogist who has a wide travelling experience in various parts of U.S.S.R. and attempts

to give a harmonious union of both ecological and historical viewpoints in explanation of the present day distribution of animals. There are four parts: (1) Introduction, (2) The conditions of existence of animals and their geographic distribution, (3) Animal dispersal and area, (4) Comparative zoögeography. A special chapter on bird distribution is written by Dr. Dementieff, an ornithologist at the Zoölogical Museum of the Moscow University. This book is worthy of every attention of field and museum naturalists in America. Numerous illustrations may be useful to those who are not familiar with the Russian language.

THE BLUE-WINGED TEAL. *Its Ecology and Management.*

By Logan J. Bennett. Collegiate Press, Ames, Iowa. $1.50. 9 x 6; xiv + 144; 1938.

This thesis is a carefully planned and well organized study of the natural history of the Blue-winged Teal (*Querquedula discors* L.). The author deserves much credit for his persistent and keen field observation, and for his critical evaluation of the data. A thorough description of the bird's characteristics, breeding range, migration, nesting and food habits, as well as proposed methods for its conservation form the body of the text. The volume contains many tables, charts and maps relating to the subject, as well as many carefully selected photographs. An extensive bibliography and an index add considerably to the value of this fine work.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 471. Abt. IX, Methoden der Erforschung der Leistungen des tierischen Organismus, Teil 6, Heft 4 (Schluss). Methoden der Meerwasserbiologie.* Containing the following articles: *Narkose und Anästhesie wirbelloser Tiere des Süss- und Meerwassers*, by Carl I. Cori; *Allgemeine Gesichtspunkte für die Einrichtung biologischer, hydrographischer Meeresanstalten und Fischereistationen*, by Carl I. Cori, Thilo Krumbach, and Hjalmar Broch.

Urban and Schwarzenberg, Berlin. RM. 9.50; (25 per cent reduction outside of Germany). 10 x 7; 135; 1938 (paper).

The first article contained in the number lists (a) various anesthetics together with their properties and their adequacy in work with invertebrate marine and fresh water animal forms, and (b) these organisms with the work which has been done upon them to show the effects of anesthetics or narcotics.

The second paper is a very detailed exposition of the structure and arrangement of marine and fishery stations as they should be and as they are.

LAC CULTIVATION IN INDIA. *Being a Second and revised edition of "A Practical Manual of Lac Cultivation" by P. M. Glover, Published in June 1931.*

By P. M. Glover. Indian Lac Research Institute, Namkum, Ranchi, Bihar, India. Rs. 2. $9\frac{3}{4}$ x $7\frac{1}{2}$; [8] + 147 + 16 plates; 1937.

This volume is a manual on the methods of lac cultivation, with its attendant problems of lac hosts, the inoculation and propagation of the lac insect, parasites and predators, the manufacture of shellac, etc. The principle value of the book will be for those concerned with the lac industry; it will also serve as a reference on the subject. It includes plates and figures, appendices and a bibliography.

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society. Volume XXIII, Part 1, Numbers 1–4.*

New York Zoological Society, Zoological Park, New York. $1.25. $10\frac{1}{2}$ x 7; 98 + 18 plates; 1938 (paper).

This number of *Zoologica* contains papers on the following subjects: (1) The significance of differential locomotor activity as an index to the mass physiology of fishes; (2) A study of the anoplocephaline cestodes of North American rabbits; (3) Penaeidae from the region of Lower California and Clarion Island, with descriptions of four new species (Templeton Crocker Expedition), and (4) Fibro-epi-

thelial growths of the skin in large marine turtles *Chelonia mydas* (Linnaeus).

Die Staaten der Ameisen.

By Wilhelm Goetsch. Julius Springer, Berlin. 4.80 gold marks. 7¼ x 4½; vii + 159; 1937.

A popular book on the wonder world of the ants, and the development of their social life. It appears in the collection *Verständliche Wissenschaft.* The author utilizes many of his own observations made in different countries of the world. A series of simple but excellent illustrations (84 in number) occur in the text but no index has been prepared.

Bienengift als Heilmittel.

By Robert Schwab. Georg Thieme Verlag, Leipzig. M. 2.40. 9⅝ x 6⅜; 48; 1938 (paper).

In this booklet bee poison is recommended in the treatment of rheumatic diseases. There are different preparations now in trade for injection as well as for embrocation. No attempt has been made by the author to prove the efficacy of this treatment by dealing with exact histories of patients or therapeutic statistics.

BOTANY

General Plant Physiology.

By E. C. Barton Wright. Foreword by Sir F. Gowland Hopkins. Williams and Norgate, London. 15s. net. 8½ x 5⅝; 539; 1937.

An Introduction to the Principles of Plant Physiology.

By Walter Stiles. Methuen and Co., London. 27s. 6d. net. 9¾ x 6½; viii + 615; 1938.

Both of these texts will be found most useful. Their authors are Englishmen—Wright, formerly Lecturer in Botany in King's College, London, is now a biochemist to the Research Association of British Flour Millers; Stiles is Mason Professor of Botany in the University of Birmingham. The first of the volumes (forward by Sir F. Gowland Hopkins O.M.)

is meant as a general survey of plant physiology for first and second year University students. It does not pretend to give the most recent information on any one particular branch; rather has the aim been to discuss the fundamental principles of the subject. A large number of different texts have been consulted and references to the literature from 1900 onwards are given on each page.

There is an appendix on "The conception of pH," a general bibliography of 87 references, and author and subject indexes. The second volume (begun ten years ago and first printed in 1936) was written especially for "University students reading for pass or honours degrees." No attempt has been made to mention all the important researches in plant physiology—only those have been referred to that are useful in emphasizing the fundamental principles. "Purely biochemical details have been introduced only so far as reference to them has appeared essential to an understanding of the physiology of the plant." The list of references to the literature in the text covers 48 pages and the index 31 pages. Both of the volumes are well illustrated with tables, graphs and figures.

Arbres et Forêts.

By Léon Pardé and Maurice Pardé. Librairie Armand Colin, Paris. 17.50 francs (cloth); 15 francs (paper). 6¾ x 4½; 224; 1938.

It is astonishing how much information the author has been able to compress between the covers of this pocket sized volume. In addition to a complete treatise on dendrology, a discussion of the effect of climatic, geological, and biological factors on the growth and well-being of trees, and of the destructive and constructive care and management of forests, the reader finds descriptions of the principal types of forests of the world, both natural and artificial, such as the coniferous forests of the northwest, the eucalypt forests of Australia, the hardwoods of the tropics, the deciduous trees of the temperate zones, the forests of the high mountain ranges of the world, all of which read like travel

talks, and makes the reader regret the absence of illustrations. Most of this descriptive material deals naturally with the forests of France, and when one recalls that this country extends from sea level on both the Atlantic and Mediterranean to the summit of Mt. Blanc, one can appreciate what a variety of forestation it contains. There is also a discussion of forestry from the commercial standpoint—lumber and paper pulp, as well as the different kinds of foods and medicines obtained from trees. The bibliography lists 48 items but there is no index.

PLANT CHEMICULTURE. *A Guide to Experiments in Growing Plants Without Soil. Second Edition.*

By C. D. Dawson and M. V. Dorn. Dawson and Dorn, Los Angeles, Calif. $1.00. $8\frac{1}{2} \times 5\frac{1}{2}$; 110; 1938 (paper).

The growing of plants without soil, both as a hobby and as an industry has attracted world-wide interest during the past few years, and if we may hazard a prediction will in short order become a well-grounded science.

The present volume is a handbook for the successful growing of plants in chemical solutions. There is a brief comment on the nutritional requirements and the physiology of plant growth, together with a sketch of the past and present history of plant chemiculture. The majority of the space is devoted to how's, why's, and wherefore's; the equipment, chemical materials, heating, lighting, and lastly, the different chemical formulae used in the process. Brief notes on planting and the treatment of pathological conditions in plants are also included. A few paragraphs on sources of supplies, equipment, and information conclude the volume.

CRYPTOGAMIC BOTANY. *Volume 1, Algae and Fungi; Volume 2, Bryophytes and Pteridophytes.*

By Gilbert M. Smith. McGraw-Hill Book Co., New York. Vol. 1, $4.00; Vol. 2, $3.00. 9 x 6; Vol. 1, viii + 545; Vol. 2, vii + 380; 1938.

In these two volumes the student who wishes to extend his knowledge of plants below the level of seed plants has a dependable guide.

> It is written from the standpoint that a thorough knowledge of a representative series in each of the major groups is better than scraps of information about a large number. . . . It is hoped that the introductory discussion to classes, orders, and families will help call attention to those characters of the selected representatives which are of distinctive importance and those which are special to the representative itself. In certain cases, as with the diatoms and the slime molds, it has been thought more advantageous to present the group as a whole instead of discussing selected representatives. . . . An attempt has been made to present both sides of the controversial subjects, but I have not hesitated to express an opinion upon the relative merits of the arguments.

Both volumes are well illustrated and indexed, and each chapter concludes with a very useful bibliography. A valuable reference work for all biological laboratories.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. Lieferung 469. Abt. XI, *Chemische, physikalische und physikalisch-chemische Methoden zur Untersuchung des Bodens und der Pflanze, Teil 4, Heft 7. Ernährung und Stoffwechsel der Pflanzen.* Containing the following article: *Die Messung der osmotischen Zustandsgrössen pflanzlicher Zellen und Gewebe, by Alfred Ursprung.*

Urban und Schwarzenberg, Berlin and Wien. RM. 24. 10 x 7; 464; 1938 (paper).

There has for some time been a confusion in the definitions of such terms as osmotic pressure, turgor pressure, turgor power, absorption power, osmotic value, cane sugar value, concentration of cell sap, etc. The author carefully explains the meaning and use of these terms before proceeding with his discussion on the most appropriate methods to be employed in the measurement of various osmotic properties of different plant cells and tissues. The bibliography covers twenty closely printed pages.

APPLIED MYCOLOGY AND BACTERIOLOGY.

By L. D. Galloway and R. Burgess.

Leonard Hill, Ltd., London. 10s. (Obtainable in North America from Chemical Publishing Co., New York. $4.00) $8\frac{1}{2} \times 5\frac{5}{8}$; ix + 186; 1937.

As the authors admit in their foreword, one cannot compress into one small volume a complete account of mycology and bacteriology, but Galloway and Burgess have done a very fair job of outlining the possible applications of the two sciences, and of indicating where more detailed information is to be found. To anyone wanting a general survey of microbiology, or a brief sketch of some particular phase of it, this book should be helpful. The bibliographies at the ends of the chapters are excellent.

AN ECOLOGICAL GLOSSARY.

By J. Richard Carpenter. University of Oklahoma Press, Norman. $4.00. $7\frac{3}{4} \times 5\frac{1}{4}$; x + 306 + [15]; 1938.

The rapid and continued growth of the science of ecology has evolved a terminology which has become somewhat complex and confused. By defining the science's numerous terms and whenever possible giving a reference to the source of the words, by listing synonyms, etc., this glossary will make the literature more comprehensible to those not well versed in ecological lingo. Several ecological maps (which are mostly out of print), classifications, and terminologies are appended.

CONSUMPTION AND PRODUCTION OF TOBACCO IN EUROPE. *U. S. Department of Agriculture. Technical Bulletin No. 587.*

By J. B. Hutson. Government Printing Office, Washington. 15 cents. $9\frac{1}{8} \times 5\frac{7}{8}$; 114; 1937 (paper).

Much useful information is given in this bulletin for those who have to deal with the tobacco question. After the World War, changes took place in the consumption of tobacco products—there was an increase in the use of cigarettes and a decline in the use of cigars. Numerous tables on the consumption and production of tobacco in all the different countries of Europe, from 1913 to 1935, are given and a map is added showing the tobacco-producing regions in every country.

PLANT ECOLOGY. *Second Edition.*

By John E. Weaver and Frederic E. Clements. McGraw-Hill Book Co., New York. $5.00. 9×6; xxii + 601; 1938.

The second edition (first edition reviewed in these columns, Vol. 5, No. 1) of this standard work has been completely revised to include the newer discoveries in the rapidly progressing field of plant ecology. The bibliography alone has been enlarged from 606 to 1035 titles.

THE VEGETATION OF CRANBROOK LAKE BOTTOM, OAKLAND COUNTY, MICHIGAN. *Cranbrook Institute of Science, Bulletin No. 11.*

By Cecil Billington. Cranbrook Institute of Science, Bloomfield Hills, Mich. 20 cents. 9×6; 20 + 1 plate; 1938 (paper).

The great mass of vegetation appearing on the dry bottom of Cranbrook Lake less than a year after drainage seems to indicate that mesophytic plant seeds becoming embedded in silt at the bottom of lakes and streams can remain dormant and viable for an undetermined number of years.

SILVA FENNICA 40. *Finnish Game and Hunting. [Suomen riista- ja metsästysolot.]*

By V. M. Klemola. Society of Forestry in Suomi, Helsinki. $9\frac{5}{8} \times 6\frac{1}{4}$; 27; 1937 (paper).

SILVA FENNICA 41. *Muhkurin Kasvisto. [Die Flora des Eichenwaldes von Muhkuri.]*

By A. V. Auer. Society of Forestry in Suomi, Helsinki. $9\frac{5}{8} \times 6\frac{1}{2}$; 36; 1937 (paper).

SILVA FENNICA 42. *Metsänhoitajien Jatkokurssit 1936 II. [Der Fortbildungskursus für Forstmeister 1936 II.]*

Society of Forestry in Suomi, Helsinki. $9\frac{5}{8} \times 6\frac{1}{2}$; 210; 1937 (paper).

SILVA FENNICA 43. *Suomen Metsätieteellisen Seuran 25-Vuotisjuhla 18. 4. 1934. [25 Years' Jubilee of the Society of Forestry in Suomi on April 18th, 1934.] [Fest zum 25-Jährigen Bestehen der Forstwissenschaftlichen*

Gesellschaft in Suomi am 18. April 1934.]
Society of Forestry in Suomi, Helsinki. 9⅝ x 6½; 65; 1937 (paper).

SILVA FENNICA 44. *Havaintoja kasvillisuuden kehityksestä Pohjois-Suomen kuloaloilla. [Beobachtungen über die Entwicklung der Vegetation auf den Waldbrandflächen Nord-Finnlands.]*
By R. Sarvas. Society of Forestry in Suomi, Helsinki. 9⅝ x 6½; 64; 1937 (paper).

SILVA FENNICA 45. *Yksityismetsätalouden edistäminen. [Befrämjandet av privatskogsbruket.] [The Promotion of Private Forestry.]*
Society of Forestry in Suomi, Helsinki. 9⅝ x 6½; 137; 1938 (paper).

BULLETIN DER SCHWEIZERISCHEN GESELLSCHAFT FÜR ANTHROPOLOGIE UND ETHNOLOGIE 1937–38. 14. *Jahrgang.*
Société Suisse d'Anthropologie et d'Ethnologie, Institut Anthropologique de l'Université, Zurich. 2 francs. 9 x 6½; 20; 1938 (paper).

MORPHOLOGY

EMBRYONIC DEVELOPMENT AND INDUCTION.
By Hans Spemann. Yale University Press, New Haven; Oxford University Press, London. $5.00. 9 x 6; xii + 401; 1938.

The term "induction" is one that can be more easily illustrated than defined. For instance, the lens of the vertebrate eye is produced from local ectodermal tissue, but is induced by proximity of the optic cup, an outgrowth of the neurenteron. There are some species of amphibia in which the lens will not be developed if the generation of the optic cup be prevented by mutilation of the neurenteron. It is also possible in some species to divert the optic cup so that the lens is induced in an abnormal locality. Finally, it is possible in some instances to remove the ectodermal tissue from the ocular region and replace it with an implant from a different individual, not necessarily of the sames species. When the lens is induced, it exhibits the characteristics of the species from which the implant was taken, even though it may not have come from the ocular region or anywhere near it.

The possibilities of this kind in experimental investigation seem to be without limit. A remarkable technique has been perfected for making transplants of embryonic tissue in the early cleavage stages, and the 508 items in the bibliography of Dr. Spemann's book bears witness to the great amount of experimentation that has been done along this line. A great quantity of this work seems to indicate that even after the embryonic tissues have become sufficiently specialized to produce the structures for which they seem to have been destined they still retain sufficient generalization to be able to generate other structures when transplanted into other positions.

It is intriguing to speculate as to what must be the structure of an embryonic cell if the goal of its development must be determined by induction of other cells in the environment. Dr. Spemann is not sympathetic to such speculation, and he criticizes with impartiality such investigators as Driesch, Gurwitsch, Weiss, Boveri, and Child without offering any substitute theory of his own. He believes the time is not yet ripe for theorizing, and that despite the quantity of observations on embryological experiments that have already been accumulated a still greater quantity must be assembled before the field can be surveyed as a whole. In the meantime the question as to the nature of fundamental cytological structure must be held in abeyance. In this connection it is interesting to recall that about a quarter century ago it was popularly believed that cancer was the result of misplaced embryonic cells. Yet Dr. Spemann made no mention of the possibility of the induction of cancer by transplantation of healthy tissue.

It is strange that such a truly significant and important book as this should have no index, except a very brief one confined to the names of authors quoted.

TISSUE REACTIONS IN BONE AND DENTINE. *A Morpho-biological Study of the Formation and the Dissolving of Bone and Dentine.*
By Åke Wilton. Henry Kimpton, London. 15s. net. 9 x 6; 194; 1937 (paper).

While attempting to use the development

of bone and dentine as indicators in vitamin studies, the author became interested in the developmental variability of these tissues. He has been led to feel that the embryological concept of progressive determination or differentiation can somehow provide an explanation for the diverse behavior of bone and dentine cells. His investigations, while working at the Umea Hospital and at the Caroline Institute (Stockholm) as a practical pathologist, have led him to a comprehensive thesis, in description of which the present monograph was written. It covers both his correlated readings (in histology, experimental embryology, pathology, tissue culture, and biochemistry) and his own studies (on normal and rhachitic human teeth and bone, on osteolysis during experimental hyperparathyroidism and scurvy in guinea pigs, and on osteolysis associated with Paget's disease and osteogenesis imperfecta in man). The text is fully illustrated with histological figures, both colored and uncolored.

The power of precipitating the matrix substances seems to be gained by a bone or dentine cell during its differentiation: with a little differentiation, collagen is precipitated; and "with high differentiation there is also conferred the power of producing substances which act as precipitants of cementing substances and lime." These processes are reversible, but only within the limits of progressive developmental determination. In young bone cells, capable of morphological dedifferentiation, the matrix may be re-dissolved, "demasking" the cytoplasm in which it was laid down. In the reticulo-endothelial cells the processes of differentiation and of matrix-precipitation seem to be less reversible, though a certain amount of "passive" bone resorption is capable of taking place. And in physiologically aging cells, capable of little or no such dedifferentiation, only "lingering" bone resorption seems to occur.

Introduction and Guide to the Study of Histology *for Students in Medical Schools and Colleges*.

By Avery E. Lambert. P. Blakiston's Son and Co., Philadelphia. $5.00. 9 x 6¼; xi + 542; 1938.

Textbook of Histology for Medical Students.

By Evelyn E. Hewer. The C. V. Mosby Co., St. Louis. $4.50. 9¾ x 7¼; xi + 365; 1938.

Both of these texts are excellent. While, in general, they cover the same ground their methods of approach differ widely. The first, expanded from the author's *Guide to the Study to Histology and Microscopic Anatomy* is a useful guide to students who are beginners in the study of medicine. The earlier volume was almost entirely in the form of directions for laboratory study. In the present volume each of the "laboratory studies" is preceded by a general discussion of the known facts concerning the structures to be studied. The work is planned to furnish a foundation for the student's further work in physiology and pathology. The references for suggested readings (only English titles are given) are designed for use as additional readings—especially in subjects that are open to question or are controversial. Both the illustrations (185 in number, some of them colored) and the index are adequate.

The second of these texts is more detailed than the first. It is the outcome of twenty-one years' experience in teaching medical students. "Emphasis is laid throughout on the 'physiological' appearances and their relation to function in contradistinction to a fixed, so-called 'normal' state. The reactions of tissues to various conditions are briefly described in order to help the student to distinguish between variations of structure that fall within physiological limits and variations that fall outside these into the realm of histopathology." The 340 illustrations (mostly original) either are microphotographs to show the general structure under a low magnification or are diagrammatic drawings. There is no list of suggested readings but there is an appendix on histological methods and a useful index.

The Essentials of Human Embryology.

By Gideon S. Dodds. Second Edition.

John Wiley and Sons, New York; Chapman and Hall, London. $4.00. 9 x 5¾; ix + 316; 1938.

Retained in this volume are all the features of the first edition (cf. Q. R. B. Vol. 5, p. 376) with the addition of new material from recent embryological work. Due to the increased knowledge of the sex hormones, the chapter entitled "Ovulation, menstruation, and pregnancy," has been rewritten. While giving an adequate presentation of the material the author has endeavored to make it clear and brief, recognizing the necessity for this in a general embryology course for medical or pre-medical students. The book treats of the germ cells, stages of development, the placenta and fetal membranes, and then gives the origin and development of the structures in each of the organ systems. An excellent group of figures and diagrams illustrates the text and the volume is well indexed.

UNTERSUCHUNGEN ÜBER DIE AUGENHÖHLEN DES MENSCHEN IN VERSCHIEDENEN LEBENSALTERN.

By Per Pallin. Isaac Marcus Boktryckeri-Aktiebolag, Stockholm. 5 Swedish kr. 107; 10½ x 7¼; 1937 (paper).

This thesis from the University of Uppsala is a report of an investigation on the evolution of the orbit from fetal to adult life. The author's material included 440 skulls at various ages from birth to adult (not including senile), and 6 fetal skulls. At the beginning of the second month of fetal life the eyes look lateral, in the third the angle is reduced to 105 degrees and at birth attains 71 degrees. It converges a further 3 to 5 degrees by maturity. The work is illustrated with photographs and diagrams, includes several tables and a bibliography of over 100 titles.

ATLAS OF CAT ANATOMY.

By David B. Horsburgh and James P. Heath. Stanford University Press, Stanford University, California; Oxford University Press, London. $1.00. 11 x 8½; 39 leaves; 1938.

All text has been omitted from this little volume on the anatomy of the cat. It consists of simple pen-and-ink drawings (42 in number), appropriately labeled, of the various systems of this common laboratory animal. There is no doubt that the atlas will prove a boon to students of cat anatomy.

THE DORSAL SPINE OF CLADOSELACHE. THE NEUROCRANIUM AND JAWS OF CLADOSELACHE. *Scientific Publications of the Cleveland Museum of Natural History, Vol. VIII, No. 1.*

By John E. Harris. Cleveland Museum of Natural History, Cleveland. 30 cents. 9¼ x 6¼; 12 + 2 plates; 1938 (paper).

This investigator reports the presence of a large spine situated in front of the first dorsal fin in two species of *Cladoselache* studied from specimens in the Cleveland Museum. This spine shows somewhat more primitive features than that in *Ctenacanthus*, thus indicating that *Cladoselache* is not the most primitive of cladodont sharks. A description of two neurocrania with their associated mandibular and hyoid arches is also given.

MORPHOLOGIE UND HISTOPHYSIOLOGIE DER NORMALEN SCHILDDRÜSE. *Zwanglose Abhandlungen aus dem Gebiete der Inneren Sekretion. Band 3.*

By B. Eggert. Johann Ambrosius Barth Verlag, Leipzig. RM. 13.50. 9⅜ x 6¼; [4] + 113; 1938 (paper).

This is a complete study of the histology, histogenesis, histophysiology, and morphology of the thyroid in both animals and men. There are 33 excellent illustrations and an extensive bibliography.

PHYSIOLOGY AND PATHOLOGY

LECTURES ON THE EPIDEMIOLOGY AND CONTROL OF SYPHILIS, TUBERCULOSIS, AND WHOOPING COUGH, AND OTHER ASPECTS OF INFECTIOUS DISEASE. *The Abraham Flexner Lectures, Series Number Five.*

By Thorvald Madsen. Published for Van-

derbilt University by Williams & Wilkins Co., Baltimore. $3.00. 8 x 5¼; xv + 216; 1937.

The first lecture deals with control of venereal disease in Denmark, with special reference to syphilis. The results are indeed remarkable, due partly to special legislation that makes treatment compulsory if necessary, and partly to the high level of education of the Danish people. The decrease, especially after the War (illustrated by a series of graphs), is more striking in Copenhagen and some other towns than in the rural districts.

In the second lecture the mechanism of bacterial infection is discussed, mostly according to investigations by Dr. Ørskov and collaborators in the Danish State Serum Institute. From these comprehensive experiments we may quote:

> In all studies on peroral infection it was found that the ways by which the infections spread were always the same, namely, by steps along the regional lymph channels of the digestive tract till the regional glands were reached. In some cases the infection did not extend any farther. Whenever there was a generalization of the infection, however, it always started from the regional lymph stations, passing along the lymphatics, reaching the blood stream by way of the thoracic duct and other centripetal lymph channels, getting thus to the organs and peripheral glands. . . . The bactericidal qualities of the blood appear not to play any particular rôle, but the fixed phagocytes, especially in the liver and the spleen play a leading part in this process.

The third chapter deals with tuberculosis in Denmark, where the mortality figure has long been the lowest of all European countries. The death-rate from tuberculosis in 1935 was 52 per 100,000, that is about the same as in the whole of the United States. Madsen is in favor of the BCG vaccination although not giving a statistical proof. He furthermore shows that bovine infection plays a considerable rôle, at least in Denmark, even in lung tuberculosis. In connection with his well-founded opinions on relative immunity in tuberculosis the following conclusion from his summary may be quoted: "At present, it seems to me, it might be too risky in a country like ours, even if it were practicable in a few places, to eradicate tuberculosis entirely, but preferably to modify the course of this infection in such a way that it becomes protective instead of destructive."

In the fourth lecture the influence of seasons on infection is illustrated for the most important infectious diseases by a great many graphs. These matters are fairly well-known although the single factors of the weather have not yet been clearly enough elucidated. In the closing chapter a comprehensive survey is given on whooping cough research, a field in which the author and the Danish Serum Institute have led for many years. Experiences in Faroe Islands and Denmark point to the conclusion that early vaccination is the most important method in the campaign against this disease. However, the author says in this remarkable study,

> . . . as long as there is no chance of our banishing these diseases from every country of the world, we must be careful not to banish them from a single country, for by so doing we run the risk of experiencing what we have observed under the measles epidemic on the Faroe Islands, namely, when a disease finally returns after a long absence, it attacks young and old without discrimination, and readily assumes that pernicious character so often observed in diseases among a "virgin population." . . . It is far better to retain the epidemics under the form of "childhood diseases" whose attacks render the entire population practically immune; . . .

THE CAUSE OF CANCER.

By David Brownlie. Chapman and Hall, London. 7s. 6d. net. 7¼ x 4¾; [6] + 208; 1938.

It is surely a curious thing when an engineer and fuel technologist—even an outstanding man in his field—gives a new theory on cancer. "The cause of cancer" is of course an important head-line, and a medical man or a biologist would hardly, at the present stage of research work, have the courage to use this label. But perhaps there is a great advantage in knowing only a little or maybe nothing of the enormous amount of clinical observation, experimental work in cancer pathology, statistics, and literature that has been done all over the world for many years past. Maybe this is the way to find out some day the real cause of malignant growths, if there is a single one.

Brownlie is of the opinion that cancer in human beings is largely due to

> complex poisonous products from high temperature carbonization and general heat decomposition, and

combustion of coal and other carbonaceous material, which largely enter the body in food, with manufactured towns gas and the smoking of meat, fish, and other products playing a primary part. Or in other words that cancer is essentially a carbonization and combustion disease. (P. 69).

The author's theory is based on the fact that the manufactured town gas, almost always coal gas, has been used more and more for general domestic and industrial purposes, in some countries more than in others, and in England especially more than in the United States. On the other hand he is of the opinion that cancer mortality has increased enormously, and most in England and Wales where the death rate is now about seven times higher than 80 years ago! This view is supported by very rough statistics and a few figures of crude mortality rates. The author has never dealt with statistics, otherwise he would perhaps know that there is no real increase shown by more precise statistics by age groups, or at most only a small one. From this point of view the new theory of the author cannot be proved true. There might, however, be some truth in supposing that certain benzene ring hydrocarbons formed by high temperature carbonization and, contained in manufactured town gas, play a part in the cancer etiology—but that should be proved very carefully by experimental work before going into a general theory. The fact that cancer occurs mostly in the higher age groups as well as in special localizations is not explained by the supposition of a simple gas poisoning.

Syphilis, Gonorrhea and the Public Health.

By Nels A. Nelson and Gladys L. Crain. Macmillan Company, New York. $3.00. 8 x 5½; xvii + 359; 1938.

This book constitutes an attempt to inform the layman about venereal diseases and the means of controlling them. In nontechnical language the authors describe the etiologic agent, the clinical signs and treatment of syphilis and of gonorrhea. In addition, they summarize what data there are relative to the incidence and distribution of these diseases in the general population. The latter part of the book is taken up by a discussion of the ways and means of successfully controlling not only syphilis, as seems to be the limited objective of the present public health program, but also gonorrhea. In their view:

> The control of the genitoinfectious diseases is a problem, not of diagnosis and treatment alone, but of discovering how to persuade people to suspect infection, seek a diagnosis and take treatment. It is a problem in how to fit a program to human nature, human weaknesses, misunderstandings, ignorance and difficulties, all mixed up with an often broken, but none the less exacting moral code. The health officer may sally forth with a badge and a club, with authority and powers, and catch a few of the less fleet-footed; but he will not control genitoinfectious diseases thereby. They may, and probably will disappear, but it will be only to get under cover.

This sane attitude is welcome and the details of the program which they present should be given serious study. But Reginald the Office Boy says that some of the bad boys he knows tell him that you don't have to persuade people to suspect infection when they have gonorrhea. They will know about it, and how!

The Brain and Its Environment.

By Joseph Barcroft. Yale University Press, New Haven; Oxford University Press, London. $2.00. 8 x 5½; xii + 117; 1938.

The scope of this book, based on the Terry Lectures at Yale delivered by Dr. Barcroft, Professor of Physiology at Cambridge University, is indicated by the headings of the three chapters which comprise the excellent content: The activity of the brain in mid-foetal life; The activity of the brain at birth; Mental efficiency considered in relation to some properties of the blood. Dr. Barcroft adheres closely to known facts, summarizing in the first chapter basic observations of his own and of other physiologists on the physiology of the embryo. The second chapter deals particularly with alterations in the oxygen supply to the brain associated with the transition from prenatal to postnatal life in the sheep. In the final chapter, the effects on the higher mental processes in man of reduction and increase in body temperature, oxygen want and excess, carbon dioxide deficiency and excess, and hypoglycemia are considered. Dr. Bar-

croft points out that experimental results lead to the conclusion that nervous function does not appear as a result of need for that function, but is present, "anticipating" the need long before it exists.

The book is an important and specific presentation of the essential work to date in a pioneer field of research. The book is indexed and appropriate references are appended to each chapter.

TUBERCULOSIS AND LEPROSY. *The Mycobacterial Diseases. Symposium Series, Vol. 1. Edited by Forest Ray Moulton. Publication Committee: Esmond R. Long, George W. McCoy, Earl B. McKinley, Malcolm H. Soule, Wm. Charles White. The American Association for the Advancement of Science, Washington, D. C.* \$2.50. $10\frac{1}{4} \times 7\frac{1}{2}$; 133; 1938.

This volume includes the papers presented at the seventh symposium (held in Denver, June 1937) organized by the Section on Medical Sciences of the American Association for the Advancement of Science. It deals with the whole complex of diseases caused by acid-fast-bacteria, among which, of course, tuberculosis still plays by far the most important part. After a short introduction on occurrence and properties of the whole acid-fast family or *Mycobacterium*—a genus of the Mycobacteriaceae—the pathology and bacteriology of tuberculosis is discussed—five general headings; tuberculosis in animals—six headings; and leprosy—six headings. A concluding section gives a summary and unification of all the different problems discussed. The volume—the work of specialists—gives a valuable insight into the present state of knowledge in pathology and bacteriology of tuberculosis and leprosy. Unfortunately epidemiology and statistics are not included in this symposium.

THE CONQUEST OF CHOLERA: *America's Greatest Scourge.*
By J. S. Chambers. The Macmillan Co., New York. \$4.75. $8\frac{1}{2} \times 5\frac{5}{8}$; xiv + 366 + 40 plates; 1938.

Asiatic cholera caused thousands of deaths in this country during the period from 1832 when it was first introduced to 1892 when the last epidemic occurred. From the official reports and from newspaper and other accounts of the period the author has ably pieced together the important epidemiologic features of this disease. In a vivid style he discusses the introduction of cholera into this country, traces its course throughout the nation, describes the futile efforts to control it and how it was stopped at last by the successful application of bacteriologic and epidemiologic discoveries. Written in non-technical language, this book should be of general interest because it presents a summary of the great progress of medicine during the 19th century and an excellent illustration of some of the immediate practical benefits derived from it.

PATHOLOGY. *Clio Medica.*
By E. B. Krumbhaar. Paul B. Hoeber, Inc., New York. \$2.00. $6\frac{1}{2} \times 4\frac{1}{2}$; xvii + 206; 1937.

The editor himself has contributed an interesting volume to the Clio Medica series on the history of medicine, and, in a departure from the usual method, has placed his emphasis on ideas and trends in the development of pathology, with the names of great men coming in only as those men influenced the field. With this in mind, a three-part division of the book is apparent, with the cell concept and its application to pathology marking the change from "the earliest halting observations," "the systematized, voluminous studies," and "the tissue concepts" to "the rational, integrated pathology" of modern times. Added to the general appeal that any brief history has, there is here, as a special attraction for the medical reader, a list of the early chairs of pathology and pathological anatomy, as well as a list of Pathologic Milestones which includes the dates of many "first descriptions." The statement in the bibliography that the Sydenham Society Publications, Old and New Series, London, 1859–1907, give good translations of many of the classics of pathology, is worth noting, too.

LE PHÉNOMÈNE DE LA GUÉRISON DANS LES MALADIES INFECTIEUSES.

By F. D'Hérelle. Masson et Cie, Paris. 75 francs. 10 x 6½; 414; 1938 (paper).

A discussion of bacteriophages and bacterial mutations is given in the first three chapters of this volume. The epidemiologist will find the second section of particular interest; that dealing with the infectious diseases studied by the author in various parts of the world over a period of twenty years. On the basis of these studies he has formulated a theory as to the manner by which such diseases may be treated and epidemics controlled through bacteriophages, which are themselves transmissable. There is an index, bibliography, and appendices which give methods of preparation of bacteriophages and determination of anti-phages.

RADIOKYMOGRAPHIE DU COEUR ET DES VAISSEAUX.

By Emile Bordet and H. Fischgold. Masson et Cie, Paris. 30 francs. 7⅞ x 5½; 134; 1937 (paper).

For anyone interested in the new method of studying the movements of the outlines of the heart, this will be a very useful little book. In radiokymography one either moves a grid in front of the film during exposure, or the film is moved back of the grid. The jagged, saw-tooth edges which then appear around the heart shadow are analyzed, often with the help of a photoelectric cell and a recording galvanometer. It is hard to say yet how much new information of value to the physiologist and clinician is going to be obtained in this way, but the method is certainly interesting. The book is interestingly written and well illustrated.

DANCE OF DEATH.

By Helen McCloy. William Morrow and Co., New York. $2.00. 7½ x 5; viii + 303; 1938.

Written in popular style, *Dance of Death* is an exciting murder mystery in which the experiments of Mayer, Magne, Plantefol, and Derrien on 2, 4 di-nitro-phenol, or thermol, play an important part. Their research on thermol may be found in the *Annales de Physiologie et de Physicochimie Biologique*, 1932, Vol. 8, pp. 1–194.

This unusual story concerns itself with an illegal exploitation by business racketeers of thermol as a reducing drug, a young woman dying from a fatal heat stroke with the temperature at nine above zero, and the solving of the crime by a psychiatrist in search of "psychic fingerprints." Of especial interest are Miss McCloy's descriptions of the autopsy and other pertinent medical and chemical facts concerning the lethal internal heat stroke caused by thermol.

METHODS OF TISSUE CULTURE.

By Raymond C. Parker. With a Foreword by Alexis Carrel. Paul B. Hoeber, Inc., Medical Book Dept. of Harper and Bros., New York. $5.00. 9¼ x 6¼; xxxii + 292; 1938.

The techniques of tissue culture in use at Carrel's laboratory at the Rockefeller Institute are lucidly described and explained in this manual. Dr. Parker's purpose is to make these reliable procedures more generally available, for in the words of Dr. Carrel, "Poor techniques always engender meaningless results."

The history of tissue culture is briefly reviewed in the foreword and in the introduction. There are 63 illustrations of cultures, special equipment, apparatus arrangements, and laboratory procedures. In the bibliography, 753 selected references covering the many diverse fields of tissue culture are listed as a general introduction to the more extensive literature available on tissue culture techniques. An important reference work.

PNEUMONIA AND SERUM THERAPY. *Revised Edition of Lobar Pneumonia and Serum Therapy.*

By Frederick T. Lord and Roderick Heffron. Commonwealth Fund, New York; Oxford University Press, London. $1.00. 8¼ x 5½; viii + 148; 1938.

This is a revised edition of a book published in 1936 by the Commonwealth

Fund. It discusses the use of antipneumococcic serum in the treatment of pneumonia of the several types, and it is full of practical and detailed information as to the technic of using these serums. It tells how to organize a program for pneumonia-control in a community, and it gives interesting and encouraging figures showing the reduction in mortality which has been obtained with serum in the several types of pneumonia. This book should be an almost necessary addition to the library of anyone engaged in this field of work.

THE PHARMACOLOGICAL SHOCK TREATMENT OF SCHIZOPHRENIA. *Revised English Edition. Nervous and Mental Disease Monograph Series No. 62.*

By Manfred Sakel. With a Foreword by Otto Pötzl. Authorized Translation by Joseph Wortis. Nervous and Mental Disease Publishing Co., New York and Washington. $2.75. 9 x 6; xviii + 136; 1938.

This book contains a detailed description of the procedure for treating schizophrenia by the insulin shock method which the author originated. The four phases in the basic method are outlined and discussed. Factors modifying the technic in the individual case are well presented, and the management of complications considered. Nine case histories are presented in detail. The book closes with a theoretical but interesting discussion of the possible mechanisms by which the therapeutic results are obtained. The material is presented without pretension, but on an optimistic note which it is hoped further experience will justify.

MÉLANGES JEAN DEMOOR. *Volume Jubilaire offert à M. le Professeur Jean Demoor à l'occasion de son élévation à l'Honorariat.*

Various Authors. Masson et Cie, Paris. 60 francs. $9\frac{3}{4}$ x $6\frac{1}{2}$; 512; 1937 (paper).

As a tribute to Jean Demoor, his colleagues and contemporaries have contributed articles which have been collected and made into this volume. Since 1886 when he first published a list of the Cicindelidae, the French physiologist has been publishing continuously; the list of his works alone covers thirteen pages.

Most of the articles are brief and of interest mainly to physiologists. The many illustrations, tables and graphs throughout the text and the reference lists which usually accompany the articles make this a really useful volume.

EAT AND KEEP FIT.

By Jacob Buckstein. Emerson Books, New York. $1.00. $5\frac{1}{2}$ x $8\frac{1}{4}$; 128; 1938.

Buckstein has here written for laymen an excellent little book on diet. The essential facts have been well chosen and well presented, and the intelligent layman who has not already read two or three books of this type will doubtless get pleasure and profit from reading this one. There are explicit directions for the use of a fourteen-day type of reduction diet, and there are lists of 100-calorie portions and of foods containing the several vitamins and minerals essential to health.

LIFE, HEAT, AND ALTITUDE. *Physiological Effects of Hot Climates and Great Heights.*

By David B. Dill. Harvard University Press, Cambridge; Oxford University Press, London. $2.50. $7\frac{7}{8}$ x $5\frac{1}{2}$; xiv + 211; 1938.

This book, based on work done in the Fatigue Laboratory of Harvard University, analyzes the physiological mechanisms by which organisms adapt themselves to life in hot climates, wet or dry, and in high altitudes. As one becomes acclimatized to heat the concentration of chloride in the sweat becomes less and the amount of sweat greater. In adaptation, both to heat and to high altitudes, there is much individual variation.

THE LIVING BODY: *A Text in Human Physiology.*

By Charles H. Best and Norman B. Taylor. Henry Holt and Co., New York. $3.60. $8\frac{1}{2}$ x $5\frac{5}{8}$; xxii + 563 + 15 plates; 1938.

An excellent physiological text, this manual is designed for use in colleges, nursing schools, dental and agricultural institutions. It is based upon the authors' former text, *The Human Body and Its Functions*. There are 298 illustrations, 15 of which are colored plates. The drawings of the anatomy and histology of organs and cells are extremely helpful for a better understanding of the discussions on normal and pathological bodily functions.

STUDIES ON THE PHYSIOLOGY OF THE EYE: *Still Reaction, Sleep, Dreams, Hibernation, Repression, Hypnosis, Narcosis, Coma, and Allied Conditions.*

By J. Grandson Byrne. Re-issue with Supplement and New Index. H. K. Lewis and Co., London. 40s. net. $9\frac{5}{8} \times 5\frac{3}{4}$; xii + 440; 1938.

This book was first reviewed in Vol. 9 (p. 115) of the QUARTERLY REVIEW. No changes have been made in this re-issue except for the addition of a supplement on the effect of stimulation of the cortex cerebri upon the effector mechanisms which mediate movements of the iris and membrana tympani. The index has been revised and much condensed.

KURZES WÖRTERBUCH ZUR GESCHICHTE DER MEDIZIN.

By B. Mayrhofer. Gustav Fischer, Jena. RM. 10.50 (bound); RM. 9 (paper). $10 \times 6\frac{3}{4}$; iv + [4] + 224; 1937.

This dictionary is useful for a brief insight into the development of medical history and science, especially from the clinical point of view. Of course it cannot be very complete, the newer developments of hygiene and preventive medicine, biometry and medical statistics being barely mentioned. Names like Galton and Pearson are entirely omitted while those of many others of much less importance in modern times are included.

TEACHING PROCEDURES IN HEALTH EDUCATION.

By Howard L. Conrad and Joseph F. Meister. W. B. Saunders Co., Philadelphia and London. $1.75. $7\frac{1}{4} \times 5\frac{1}{4}$; 160; 1938.

Teachers and student-teachers of hygiene in secondary schools might find this text an aid in their work. Various methods to stimulate learning are presented not only by actual application of health procedures by the student, but also by planned lessons, readings, and oral and written tests.

THE TRUTH ABOUT VIVISECTION.

By Sir Leonard Rogers. J. and A. Churchill, London. 5s. net. $7\frac{3}{4} \times 5$; x + 182; 1937.

The pros and cons of vivisection as presented before the Royal Commission of 1906 are presented here along with evidence of later date. Although in his preface the author states that all inquirers may form their own conclusions, sensible people will be in no doubt about the correct one.

BIOCHEMISTRY

SPECTROSCOPY IN SCIENCE AND INDUSTRY. *Proceedings of the Fifth Summer Conference on Spectroscopy and Its Applications Held at the Massachusetts Institute of Technology, Cambridge, Massachusetts July 19–22, 1937.*

A Publication of the Technology Press, Massachusetts Institute of Technology. John Wiley and Sons, Inc., New York; Chapman and Hall, Ltd., London. $3.00. 10×7; vii + 134; 1938 (paper).

Within this volume twenty-nine short papers are presented, all of which were selected from the 1937 summer conference on spectroscopy. These papers give the practical applications of spectroscopy to the fields of chemistry, metallurgy, electricity, botany, experimental physiology, and medicine. A few of particular interest to biologists are the following: The analysis of the skin and urine for traces of lead; The importance of the spectrograph in establishing the diagnosis of argyria or silver poisoning; Spectrophotometric studies in the metabolism and molecular structure of hemoglobin derivatives; Some applications of spectroscopic

methods in experimental physiology; and On the origin of light-sensitivity in seeds.

A list of references is given at the end of most sections, and the entire volume is well indexed.

STUDIES ON BIOLOGICAL OXIDATION AND SOME OF ITS CATALYSTS (*C_4 Dicarboxylic Acids, Vitamin C and P, etc.*).

By Albert v. Szent-Györgyi. Karl Rényi, Budapest; Johann Ambrosius Barth, Leipzig. RM. 6.50 (bound); RM. 5 (paper). $9\frac{1}{2} \times 6\frac{3}{4}$; 98; 1937.

The author sums up his extensive research on the mechanism of the seemingly elementary reaction, $2H + O = 2H_2O$, the study of which has led to such divers results as the discovery of the catalytic nature of C_4 dicarboxylic-acids, the identification of ascorbic acid as vitamin C, and the discovery and isolation of vitamin P.

The principles and theories of biological oxidation in both plants and animals are outlined and carefully analyzed. The author's work in biochemistry has led him to conclude that ". . . there can be no real difference in the fundamental chemical mechanisms of plants and animals." He further adds, "The existence of vitamins is convincing evidence on this line, as it shows that plants and animals work with the same cogwheels."

TRAITÉ DE CHIMIE ORGANIQUE. *Tome VIII. Fascicule I et II. Quinones. Cétènes. Aldéhydes-Alcools et Cétones-Alcools. Oses et Holosides. Généralités sur les Hétérosides. Amidon. Cellulose. Lichénine. Soies Artificielles. Industries des Matières Amylacées. Industries des Sucres. Carbonisation des Bois. Aldéhydes-Phénols et Cétones-Phénols.*

By J. Amiel, M. Battegay, P. Baud, G. Champetier, R. Dolique, J. Duclaux, M. Frèrejacque, V. Hasenfratz, H. Hérissey, A. Hieulle, J. Lichtenberger, I. Marszak, H. Pariselle, J. Rabaté, M. Schoen, M. Sommelet, R. Sutra. Published under the direction of V. Grignard, G. Dupont, R. Locquin and Paul Baud. Masson et Cie, Paris. 335 francs (paper); 375 francs (cloth). $10 \times 6\frac{1}{2}$; xix + 1256; 1938.

This is the sixth volume to appear of a 15 volume treatise on organic chemistry. Preceding parts have been noticed in these columns. The high standards of scholarship and usefulness characteristic of the work as a whole are well maintained in this volume.

THE BIOLOGICAL STANDARDIZATION OF THE VITAMINS.

By Katharine H. Coward. William Wood and Co., Baltimore. \$4.50. $8\frac{1}{2} \times 5\frac{1}{2}$; viii + 227 + 7 plates; 1938.

This is a manual on the quantitative determination of vitamins in foods, preparations for therapeutic use, and products obtained in chemical investigations of vitamins. Both the practical and mathematical aspects of the subject are adequately treated. A valuable reference work.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung* 470. *Abt. V. Methoden zum Studium der Funktionen der einzelnen Organe des tierischen Organismus, Teil* 10, *Heft* 8 (*Schluss*). *Allgemeine und vergleichende Physiologie* (*Ergänzung zu Abt. V., Teil* 2). Containing the following article: *Stufenphotometrische Methoden der Blut- und Harnanalyse*, By Carl Urbach.

Urban und Schwarzenberg, Berlin. RM. 13.50; (25 per cent reduction outside of Germany). 10×7; 175; 1938 (paper).

This *Lieferung* describes gradation photometric methods utilized by various workers in the determination of 24 chemical constituents of the blood and 15 of the urine. Results for each are presented in tabular form.

SEX

YOUTH AND SEX. *A Study of 1300 College Students.*

By Dorothy D. Bromley and Florence H. Britten. Harper and Bros., New York and London. \$3.00. $8\frac{3}{4} \times 5\frac{3}{4}$; xiii + 303; 1938.

This is the report of a unique investigation

concerning the sex mores of college students. The authors visited 46 colleges and universities, personally interviewed 276 students of both sexes and in addition obtained answers to a prepared questionnaire from 1088 others. They found that about 50 per cent of the males and 25 per cent of the females had had premarital intercourse. The authors classify the students into several groups according to their attitude and behavior and discuss the characteristics of each in some detail with illustrative case histories. In the opinion of the authors the main conclusion to be drawn from their investigation is that the youngsters are keenly aware of the problems of sex and eager to learn how to solve them, but demonstrate the insufficiency of what instructions on the subject they may have received. Since this represents the first attempt to obtain factual information on the subject little can be said regarding the significance of the findings. On the basis of these data no one can justly say whether or not this generation is going to the dogs and the authors wisely avoid any conclusion on this point. There is a foreword by Raymond Pearl.

THE ART AND SCIENCE OF MARRIAGE.
By Esther B. Tietz and Charles K. Weichert. With an Introduction by Morris Fishbein. Whittlesey House, London. McGraw-Hill Book Co., New York. $2.50. 8 x 5½; viii + 279; 1938.

One might call this book a fundamental course in the physiology of the reproductive system. It also touches upon the anatomy and physiology of other parts of the body, thus leading one by a somewhat circuitous path around to the so-called science of marriage. As for the discussion of artful tactics, the descriptions which the authors give in the first fifty pages differ little from what may be found in other books on sex and marriage.

SEX SATISFACTION AND HAPPY MARRIAGE.
By Rev. Alfred H. Tyrer. Foreword by Robert L. Dickinson. Emerson Books, New York. $2.00. 5¼ x 7⅞; 160; 1938.

"This book has its source in the fountain of bitter sorrow and the remembrance of scalding tears." Reverend Tyrer, along with the scores of other sex handbook writers, has undertaken his work with the deepest sense of responsibility. He has produced a manual for use by engaged couples and also "for use in their work by doctors, the clergy, social workers, lawyers, and others in the advisory professions, who may find recommendation of this book a convenient and time-saving method of imparting advice."

BIOMETRY

STATISTICAL METHODS. *Applied to Economics and Business. Revised.*
By Frederick C. Mills. Henry Holt and Co., New York. $3.75. 8½ x 5⅝; xix + 746; 1938.

The principal feature of this revision of Mill's well-known textbook is the addition of chapters on the newer biometric approach to problems of sampling and analysis of variance. Otherwise the order of exposition in this edition is the same as in the previous one. After an introductory chapter on statistical methods in economics and business, those that follow concern graphic representations, averages, measures of dispersion, index numbers, curve fitting, correlation and elementary probability. In the appendices the author describes the least square method, the binomial distribution and special curves such as that of Gomperz and the logistic. Useful tables are also included. From this brief outline it is seen that the book contains information on practically every aspect of elementary statistical technique. The method of exposition is unusually clear and the illustrative examples well chosen.

LA DURÉE EXTRÊME DE LA VIE HUMAINE. *Actualités Scientifiques et Industrielles, 520. Statistique Mathématique, II.*
By E. J. Gumbel. Hermann et Cie, Paris. 18 francs. 10 x 6½; 65; 1937 (paper).

The author outlines a method of determining the range of variability of the

oldest age that can be attained by man. He first postulates that there is no definite limit to the possible age that can be reached, i.e., that the upper extreme of the mortality curve approaches zero only asymptotically. Then, by means of simple and elegant theorems on probability he arrives at a measure of the expected deviations of the highest age. This measure is a function of the size of the sample but is independent of the form of distribution of the deaths at the oldest age. It is computed from the usual constants of a life table. Applications of the method is made to data for Sweden, Switzerland, United States, India and Australia. A great deal of ingenuity has been demonstrated in the development of this type of analysis but whether it can be successfully utilized in the study of the biology of death remains to be seen.

THE KELLEY STATISTICAL TABLES.

By Truman L. Kelley. The Macmillan Co., New York. $4.50. 11 x 8½; [6] + 136; 1938.

The Table of Contents of this book will furnish the reader, particularly the statistician, with a general idea of its usefulness: Construction and accuracy of tables; Uses of tables; Table I. Eight-place normal distribution, simple correlation, and probability functions; Table II. Four-place χ^2 functions; Table III. Ten-place cubic interpolation coefficients; Table IV. Ten-place quintic interpolation coefficients; Table V. Eleven-place septic interpolation coefficients; Table VI. Eight-place square roots; Table VII. Constants frequently needed.

FREQUENCY CURVES AND CORRELATION. *Third Edition.*

By W. Palin Elderton. University Press, Cambridge; Macmillan Company, New York. $3.75. 8½ x 5½; xi + 271; 1938.

This is the standard text-book on the Pearsonian system of frequency curves. In this third edition (second edition reviewed in Vol. III, No. 2) the chapters on standard errors, the test of goodness of fit, the correlation ratio and contingency have been rewritten.

PSYCHOLOGY AND BEHAVIOR

PSYCHOLOGY DOWN THE AGES. *In two volumes.*

By C. Spearman. Macmillan and Co., London. $7.50 per set. 8¾ x 5⅝; Vol. 1, xi + 454; Vol. 2, vii + 355; 1937.

This is a praiseworthy and, to the layman, certainly a most welcome attempt to clarify the progress of scientific psychology. "We will consider what wisdom it has through all the ages attained, accumulated, and preserved. In particular, we will see how far and in what direction it has gone beyond the confines of common sense—thereby meaning the knowledge shared by the generality of mankind and not confined to mental specialists."

The book is divided into five main sections. Section A deals with the development of the science of the *psyche*, and this is in large part historical. Section B discusses "the main criticisms of the attempts to construct psychology along the 'oligarchic' patterns of 'faculties' or 'powers'. Most of these criticisms—notably, that these faculties have been fantastically regarded as so many substances or agents—would appear to be devoid of foundation." Section C examines "The history of psychological progress in respect of the constitution of the psyche . . ." Section D inquires into the laws governing the *psyche*, and Section E endeavors to find out individual differences—"not that wherein people agree, but that wherein they differ." This latter section is in many ways the most interesting. The author discusses correlation coefficients, the discovery and nature of the factor "G" discovered by himself and his coworkers, specific factors of ability, and orectic factors. For a more intelligible discussion of the factors G (general intelligence) and S (specific intelligence) the reader would probably do better to consult a former book of the author's—*Abilities of Man*. It is interesting to note that (page 287):

the general tide of psychology seems to have arrived at conceiving the principle of mind, the "psyche,"

as an Individual who Feels, Knows, Acts; who does so in a manner more or less well adapted to three intricately combining and often conflicting tasks; those of preserving Himself, his Family and his Society. . . . And so, after two thousand years of study, we might seem to come to a Mind which—save for the larger credit allowed to evolution—is disconcertingly similar to what it was originally supposed to be by common sense.

Obsessions and Convictions of the Human Intellect.

By F. W. Westaway. Blackie and Son, London and Glasgow. 10s. 6d. net. $8\frac{1}{2} \times 5\frac{3}{4}$; xi + 528 + 4 plates; 1938.

This book calls to mind the words of Prince John at the revivification of Falstaff, "That is the strangest tale that e'er I heard." Not only are many strange incidents of historical interest recounted here, but the book itself is such a strange mixture of profound erudition and egregious bosh that the reader can hardly believe that it all came from the same pen. It is impossible to speak of the work as a whole; the topics cry for separate treatment, and such treatment is of course precluded by lack of space.

The author is weakest when he discusses the fourth dimension. Like Lord Kelvin, he refuses to believe anything of which he cannot see a working model. The fourth dimension is therefore a figment of the physicists and must be rejected *in toto*. Space of three dimensions was good enough for Euclid and the author finds it good enough for him. There is no space-time continuum, only space and time continua, the one with three and the other with one dimension respectively.

This argument ought to make the author reject also the modern theory of atomic structure, for it teaches that the subatomic particles are fundamental entities that manifest themselves at one time as matter and at another as energy, and certainly no one could construct a working model of such a particle. Yet he accepts this theory—a procedure which is reminiscent of straining out gnats and swallowing camels.

The author's discussion of perpetual motion and of squaring the circle both suffer from misplaced emphasis. The fallacies which he exposes are more or less obvious, but there are other fallacies which are not so obvious and which he ignores, of which the explanations might be read with profit.

On the other hand, his treatment of such subjects as witchcraft and alchemy is much happier. While these topics do not come strictly within the scope of science they have a scientific aspect. We are apt to think of these as digressions from the straight and narrow path of scientific progress, as if they were abnormalities that might have been avoided. Yet we learn here that both were the inevitable outcome of the trend of past centuries. The author points out that persecutions for witchcraft were confined almost exclusively to countries where the church had been disestablished, thus supplying aid and comfort from an unexpected source to the advocates of antidisestablishmentarianism.

Finally, to the treatment of metaphysics and theology must be accorded really high praise, though even here the critic who enjoys hunting for flaws will be successful, for the author apparently believes that the Apostle's Creed of modern Protestantism is identical with that of the same name urged on the Council of Nicaea by Eusebius. But this is a detail of little importance, as it detracts nothing from the conclusions finally drawn by the author. If the chapters on physical science could have been omitted the bulk of the work would have been reduced a third, and its value and influence would have been enhanced greatly. All told, this is really a very remarkable book, despite the criticism we have given it.

Water-Divining. *New Facts and Theories. By Theodore Besterman. Methuen and Co., London.* 7s. 6d. net. $7\frac{3}{8} \times 5$; ix + 207 + 2 plates; 1938.

As the sub-title states this current volume augments all that was written in 1926 on this highly controversial subject by the late Sir William Barrett, F.R.S. and the present writer. The new evidence consists of authentic records and detailed reports of the work of various dowsers, in different parts of the world, whose

successes are demonstrable. Personal accounts of how certain dowsers react physically and psychically when actively engaged in their profession, supply much needed information. The verdict is still divided, however, between those who believe their ability to be a physical reaction to the "feel of water" in the immediate proximity, and those whose stimulus and direction seems to come from a psychical impulse. Further new evidence includes the use of the dowsing method in fields other than for locating supplies of water. It is used in locating oil and in gold mining. It is also stated that this method is being experimented with in an effort to employ it in determining the location of obscure ailments of the human body.

To establish dowsing more firmly as a science a section of the book is devoted to "theory." French and German authorities on dowsing are there quoted. There is also a description of the instruments used by dowsers, other than the classical forked twig. These include the rod made of a variety of materials, and the pendulum, and "regulable pendulum" which has been perfected in France.

The author's solid belief in dowsing is evident. He thinks that the dowser is successful because he has "other than normal" powers.

The final chapter on the "Folklore of Dowsing" is a delightfully written historical sketch concerning the magician's wand, and the Divining Rod and popular beliefs adhering to these ancient instruments in many countries.

Plates and "text plans" are used to illustrate the book and there is a three page index.

A Further Analysis of Reasoning in Rats. *II. The Integration of Four Separate Experiences in Problem Solving. III. The Influence of Cortical Injuries on the Process of "Direction." Comparative Psychology Monographs. Volume 15, Number 1. Serial Number 73.*

By Norman R. F. Maier. Johns Hopkins Press, Baltimore. $1.50. 10 x 6¾; 80 + 5 plates; 1938 (paper).

The Effect of Early Inanition upon Maze Learning in the Albino Rat. *Comparative Psychology Monographs. Volume 15, Number 2. Serial No. 74.*

By William C. Biel. Johns Hopkins Press, Baltimore. 75 cents. 10 x 6¾; 33; 1938 (paper).

In the first paper Maier shows, by a series of well organized and carefully controlled experiments, that white rats are capable of integrating several experiences and using them, to some advantage at least, in problem solving. Analysis of the scores shows that the rats made 55.0 per cent completely correct responses when chance expectation would be 25.0 per cent. The second paper deals with the influence of cortical injuries on the process of "direction" in white rats. The data here show that cortical injury probably has not as great an effect on the reaction to a problem as does preoperative training. Both papers present extensive bibliographies, graphs and tables, and the latter is illustrated with 5 plates.

Biel reaches the general conclusion that inanition, whether begun soon after birth, or in a later period of life preceded by normal rearing, causes no loss in maze learning ability as measured by error, trial, or time scores on the Warden U-maze or the Stone multiple-T maze. The paper contains many tables and graphs concerning the scores of the animals, and a bibliography of 46 titles.

The Mentally Ill in America. *A History of Their Care and Treatment from Colonial Times.*

By Albert Deutsch. With an Introduction by William A. White. Doubleday, Doran and Co., Inc., Garden City. $3.00. 8½ x 5½; xvii + 530; 1938.

This excellent and detailed history of the ideologies which have determined the care and treatment of the mentally ill in America from colonial times to the present day is a valuable and much needed contribution to the literature of psychiatry. The book begins with the days when the mentally ill were believed possessed of demons and treated by exorcism or hung as witches. It continues, describing how they came to be regarded as sub-human

beings chained in kennels and whipped; how, later they were sold as paupers on the auction block. The early establishment and later growth of state hospitals and state care is well summarized. Chapters are devoted to Benjamin Rush, the first great American psychiatrist; to Dorothea [*not* Dorothy] Dix; and, of particular interest because not so well-known, to thirteen of the hospital superintendents of the mid-nineteenth century whose vigorous thinking and activity resulted in the founding of the organization now known as the American Psychiatric Association. A thorough and directed study of the problems confronting psychiatric practise, therapeutic, administrative, legal, etc., is made; a chapter is devoted to the problem of mental defectives; also, to the growth of the mental hygiene movement; and the early relationship between American and European psychiatry is indicated. The bibliography is excellent and the book is well indexed.

THE MIND OF PRIMITIVE MAN. *Revised Edition.*

By Franz Boas. The Macmillan Co., New York. $2.75. $7\frac{3}{4} \times 5\frac{3}{8}$; x + 285; 1938.

The white races today tend to view with superciliousness the cultural achievements of other races. But in 2000 or 3000 B.C. the Egyptians had the same attitude towards what *they* regarded as primitive peoples. Some of these primitives then were the lineal ancestors of our own very important selves. In view of historical changes in the cultural status of races, the degraded position of today's primitives does not necessarily seem a permanent one. Since man is considerably over one hundred thousand years old, "What does it mean, then, if one group of mankind reached a certain stage of cultural development at the age of one hundred thousand years and another at the age of one hundred and four thousand years?"

Following these lines of thought the author penetratingly attacks such general assumptions as that "achievement depends solely, or at least primarily, upon innate racialability"; that "race and culture must be intimately associated"; and that "strangeness of type and low intelligence go hand in hand." These Boas regards as reasonings based on ignorant, emotional, and political prejudices. In the final chapter, the race problem in modern society is considered with frank impartiality.

PRIMARY MENTAL ABILITIES.

By L. L. Thurstone. University of Chicago Press, Chicago. $2.00. $9\frac{3}{4} \times 6\frac{3}{4}$; ix + 121; 1938 (paper).

This book is an application of the theory of factor analysis described in the author's *The Vectors of Mind* in 1935. The problem of factorial analysis, beginning with the record of objective performances of individuals, is to isolate and describe fundamental abilities. In this study 56 psychological tests were given to each one of a group of 240 volunteers. The nature of these tests and the statistical methodology used in analyzing the results are discussed in some detail. This psychometric approach to the elusive data of psychology is too new to have yielded clear-cut results as yet. As an objective approach to the problems of psychology it is to be commended. Actual test scores are tabulated in the appendix, and there is an index.

EXPERIENCE AND PREDICTION. *An Analysis of the Foundations and the Structure of Knowledge.*

By Hans Reichenbach. University of Chicago Press, Chicago. $4.00. $8\frac{1}{2} \times 5\frac{3}{4}$; x + 410; 1938.

Led by "the conviction that the key to an understanding of scientific method is contained within the probability problem . . .," the author presents a combination of his investigations on probability with the concept of "logistic empiricism." He proposes that the form of this new philosophical movement should be "probabilistic empiricism." The author repudiates the positivistic conception of the external world and considers probability meaning within the framework of the functional theory of meaning. Knowl-

edge he interprets as a system of posits or wagers.

THE CONTRIBUTION OF ALFRED ADLER To Psychological Medicine. *The Study of Organ Inferiorities. The Subject of the Relation of the Sexes. General Medicine. Also; The Philosophic Environment of Adler's Contribution. Child Guidance in Association with the Teacher. Individual Psychology and Adler.*

By Philip Mairet, Sir Walter Langdon-Brown, H. C. Squires, Cuthbert Dukes, O. H. Woodcock, and others. C. W. Daniel Co., London. 2s. 6d. net. $8\frac{1}{2} \times 5\frac{5}{8}$; 76; 1938 (paper).

This pamphlet, published as a tribute to Alfred Adler, contains seven articles by personal associates. The first article is a presentation of the philosophical background of Adler's teachings. The next four deal with Adler's contributions to various aspects of psychology and medicine. The sixth paper concerns Adler's contributions towards the understanding and guidance of problem children. The last paper is more or less an appreciation of Adler as a human being. All of these papers are extremely laudatory and possibly may arouse a certain amount of irritation among those inclined to evaluate the distinguished propositus more dispassionately.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

THE EVOLUTION OF PHYSICS. *The Growth of Ideas from Early Concepts to Relativity and Quanta.*

By Albert Einstein and Leopold Infeld. Simon and Schuster, New York. $2.50. $8 \times 5\frac{1}{2}$; x + 319 + 3 plates; 1938.

The first great triumph of physics, Newton's analysis of the motions of the planets, depended on the assumption of a force between particles, acting in the line joining them and depending only on their distance from each other. With the development of knowledge of electric and optical phenomena, however, the attempt to interpret them by means of mechanical concepts met with grave difficulties. A moving electric charge acts on a magnet but the force, instead of acting in the direction joining the charge and the magnet, acts in a direction at right angles to it. Furthermore the magnitude of the force, instead of depending only on the distance between the charge and the magnet, depends also on the velocity of the charge. In order to interpret these new phenomena Faraday and Maxwell introduced the concept of an electromagnetic field, the concept of an orderly system of forces extending throughout the space around the charge or the magnet.

The special theory of relativity, which applies only to systems in which Newton's law of inertia is valid, is based on two fundamental assumptions: (1) physical laws are the same in all systems moving uniformly relative to one another: (2) the velocity of light always has the same value. The general theory of relativity applies to all systems, whether inertial or not.

The older physics regarded matter as made up of elementary particles but energy was conceived as continuous. Later results have shown that the latter too is discontinuous in structure.

> Is light a wave or a shower of photons? Is a beam of electrons a shower of elementary particles or a wave? These fundamental questions are forced upon physics by experiment. In seeking to answer them we have to abandon the description of atomic events as happenings in space and time, we have to retreat still further from the old mechanical view. Quantum physics formulates laws governing crowds and not individuals. Not properties but probabilities are described, not laws disclosing the future of systems are formulated, but laws governing the changes in time of the probabilities and relating to great congregations of individuals.

Apparently Royce, the advocate of the statistical method as the fundamental description of reality, was a prophet of modern physics.

"SO YOU THINK IT'S NEW."

By Wilfred J. Funk. Drawings by Russell Sherman. Funk and Wagnalls Co., New York and London. $2.00. 8×6; x + 198; 1937.

This delightful little volume should be in every man's library no matter what his age or occupation. In these days of new deal, new social experiment, new this and that, it is refreshing to learn that there are still some persons well enough acquainted with the history of the past to realize that the contemporary sometimes suffers from delusions in evaluating the contributions of his age. In a light and thoroughly enjoyable vein the author discusses many topics from the art of make-up to that of graft, and shows the close resemblance between the present and the remote past. The student of biological sciences will find particularly amusing the verbal coincidence in the advice on the care of infants as given by Soranus, the Greek physician of imperial Rome, and by one of the most eminent pediatricians of this country.

THE INTELLIGENT INDIVIDUAL AND SOCIETY.

By P. W. Bridgman. The Macmillan Co., New York. $2.50. $8\frac{1}{2} \times 5\frac{3}{4}$; vi + 305; 1938.

The distinguished physicist who writes this book has been thinking rather seriously about the relation between the individual and society. In this exposition of his thoughts on the subject he introduces the reader first of all to the operational methods used in modern physics. Then he proceeds to analyze social behavior more or less according to these methods. He finds as a result, as many would expect, that man's social behavior is not often rational. In view of this, the author proposes as a remedy to the ills of society and a means of happiness that (1) the fact of the irrationality of social conduct should be realized by all; (2) the golden rule in a form modified by him should be universally applied; and (3) force should be used to bring in line those who do not act by this golden rule. Engagingly written, the book is pleasant to read and contains a number of bright if not entirely original ideas. It would seem that in matters regarding social behavior even the intelligent individual is at a loss since the best he can do is to propose cures which, sad to say, have already proved unworkable.

SCIENTIFIC ILLUSTRATION.

By John L. Ridgway. Stanford University Press, Stanford University, California; Oxford University Press, London. $4.00. $10 \times 6\frac{1}{2}$; xiv + 173; 1938.

It is a well-known fact that a simple drawing will produce a greater effect than a lengthily written delineation. Unfortunately too many authors interpret this idea literally and become satisfied that they are doing their duty by merely inserting illustrations of any sort, no matter how poor they may be. The purpose of this book is not to teach free-hand drawing or cartography, but to show which of the various methods of illustration are most appropriate and how the best effect may be obtained from them. The author has had much experience and he covers the field thoroughly. The scientific writer who desires to illustrate his article will find all the information he needs and the professional artist will likewise derive benefit from this comprehensive and profusely illustrated manual.

INTERNSHIPS AND RESIDENCIES IN NEW YORK CITY, 1934–1937. *Their Place in Medical Education.*

Report by The New York Committee on the Study of Hospital Internships and Residencies. The Commonwealth Fund, New York; Oxford University Press, London. $2.50. 9×6; xxx + 495; 1938.

This book is the result of a comprehensive study made by a joint committee representing the College of Physicians and Surgeons of Columbia University, Cornell University Medical College, Long Island College of Medicine, New York Medical College and Flower Hospital, New York University College of Medicine, and the New York Academy of Medicine, and financed by a grant from the Commonwealth Fund. The study represents a thorough analysis of internships and residencies in 77 hospitals in New York City, and the resulting data are presented

as a basis for determining how best the internship years can be made to supply a high standard of training adequate to the present day practise of medicine. The experience of previous interns is reviewed, the educational value of the internship is discussed, record keeping, health conservation of the house staff, hospital libraries, desirable activities of internship and residency years, and certification in the specialities are considered. The book will be of value and interest not only to medical school and hospital administrators but to medical students with internships still to be decided on, and to interns and residents whom the subject matter most closely touches. There is a bibliography and the book is indexed.

SMOKE REDUCTION. *A Standard Instruction.*

By R. S. Gill. The University Club, Baltimore. $5\frac{1}{2}$ x 4; 14; 1937.

This booklet, concerned with smoking among office gals (especially at inopportune times) suggests a method whereby the situation can be effectively handled. Unfortunately the author does not report concrete results.

THE PRICES OF BIOLOGICAL BOOKS IN 1938

By RAYMOND PEARL AND MAUD DeWITT PEARL

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WHEN the QUARTERLY REVIEW OF BIOLOGY began publication in 1926 the custom was inaugurated of reporting at the end of each volume on the cost of the books that had been reviewed in its columns during the year. The present paper, therefore, is the thirteenth of these reports on the cost of biological books. The prices of foreign books have been converted into dollars on the basis of the exchange at the time the books were received. Table 1 shows the findings for 1938, arranged in the customary manner.

The total number of pages reviewed in 1938 is 149,046, an increase of 8,686 pages, or 6.2 per cent over 1937 and an increase of approximately 81 per cent over 1926, the year in which these tabulations began. In the thirteen years of the QUATERLY REVIEW's history the books reviewed in these columns have aggregated a total of 1,624,664 pages. To American buyers these cost in the aggregate a total of $17,609.43, leading to an average price per page for the total of 1.084 cents. The weighted average cost of 1.024 cents per page for all the books reviewed in 1938 is 6.1 per cent lower than that for all the books reviewed in our columns during the preceding twelve years 1926–37 inclusive, taken as a bulk total. It is even lower than the corresponding average for 1937 of 1.053 cents per page by 2.7 per cent. The 1938 average price per page for all books reviewed is 6.7 per cent lower than the corresponding figure for 1926, which was 1.097 cents. The general picture presented by the 1938 summary is clearly of a continuation of the lowered prices that got under way in the 1937 report. The American biologist buying books in 1938 got off relatively easily, as compared with other years, so far as may be judged by our review sample. From his point of view a further continuation of the trend of these last two years would be a real and undisguised blessing.

In 1938 Germany stayed in her customary position at the head of Table 1, as the source of origin of highest prices for biological books. And, in general, all the sources of origin stood this year in about their usual order. It does not appear that the announced policy of the German publishers relative to book prices that has been discussed in these columns in recent years has produced much of a realistic effect in lowering average prices. Quite on the contrary Germany's average price per page in 1938, as judged by our sample, was actually 19 per cent higher than in 1937. There may be some deep and subtle economic principle of a beneficent character involved in the price policy of the German publishing trade, but if so it is hidden from the ordinary mortal's understanding. There would seem little doubt that the relatively high prices of German biological books, and the generally upward trend of those prices during

TABLE 1

Prices of biological books, 1938

ORIGIN	TOTAL PAGES	TOTAL COST	PRICE PER PAGE
			cents
Germany	8,025	$185.90	2.32
Other countries	2,550	55.49	2.18
British-American	12,351	142.95	1.16
Great Britain	13,685	142.44	1.04
United States	95,022	909.86	0.96
France	10,630	76.84	0.72
British Government	383	1.74	0.45
U. S. Government	6,400	10.60	0.17
Totals and weighted average, 1938	149,046	1,525.82	1.024
Totals and weighted average, 1926–1937 incl	1,475,618	16,083.61	1.090

the last thirteen years, has definitely and considerably curtailed and restricted the sale of such books in the United States. Quite apart from any question of correlatively diminished intellectual influence, this would seem a poor way to make money.

Following the custom inaugurated two years ago Table 2 shows the price trends of books published in various countries during the decade from 1929 to 1938 and the absolute and relative changes in price from 1937 to 1938 and from 1929 to 1938.

TABLE 2

Comparison of the prices of biological books for the decade from 1929 to 1938

ORIGIN	AVERAGE PRICE PER PAGE										CHANGE + OR − FROM 1937 TO 1938		CHANGE + OR − FROM 1929 TO 1938	
	1929	1930	1931	1932	1933	1934	1935	1936	1937	1938	Absolute	Relative	Absolute	Relative
	cents	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*per cent*	*cents*	*per cent*
British-American	1.90	1.91	2.27	1.48	1.29	1.45	1.53	1.81	1.44	1.16	−0.28	−19.4	−0.74	−38.9
Other countries	1.68	0.97	1.53	1.02	0.85	0.86	1.20	2.26	0.86	2.18	+1.32	+153.5	+0.50	+29.8
Great Britain	1.29	1.13	1.19	0.89	0.66	0.96	0.84	0.94	1.27	1.04	−0.23	−18.1	−0.25	−19.4
United States	1.14	1.09	1.05	1.00	1.02	0.93	0.90	1.03	0.93	0.96	+0.03	+3.2	−0.18	−15.8
Germany	1.65	1.82	1.75	1.60	1.43	1.89	2.04	1.84	1.95	2.32	+0.37	+19.0	+0.67	+40.6
British Government	0.39	1.19	1.03	1.45	1.39	0.89	0.50	1.62	0.34	0.45	+0.11	+32.4	+0.06	+15.4
France	0.47	0.47	0.69	0.60	0.74	1.00	0.86	1.05	0.85	0.72	−0.13	−15.3	+0.25	+53.2
U. S. Government	0.23	0.30	0.28	0.36	0.17	0.18	0.11	0.21	0.16	0.17	+0.01	+6.3	−0.06	−26.1

The average prices per page of our samples of biological books from every origin except Great Britain, British-American, and France were *higher* this year than in 1937, by amounts ranging from about 3 per cent for books commercially produced in the United States to 153 per cent for the "Other countries" publications. In spite of the fact that only three sources of origin showed lower prices in 1938 than in 1937 when considered separately, the importance of these sources together with other changes in the volume weighting led to the general average for all books together being lower this year than last, as has already been seen from Table 1. The 1938 average price per page for books commercially published in the United States was 15.8 per cent lower than that shown by our 1929 sample. The rise in the per page price of biological books published in France, which has been commented on in these notes in recent years, did not continue in 1938. Instead the 1938 average price was 15.3 per cent lower than the 1937, and absolutely fell back to the level characteristic of the years around 1931–33. Our sample of biological books from British commercial publishers showed a 19.4 per cent drop in price from the 1937

sample. German books, as judged by our samples, increased in price in 1938 to American biologists, by 19.0 per cent over 1937, and by 40.6 per cent in the last decade.

Table 3 sums up the whole thirteen years experience of the QUARTERLY REVIEW.

TABLE 3

Average biological book prices over the thirteen year period, 1926–1938 inclusive

ORIGIN	TOTAL PAGES	AVERAGE PRICE PER PAGE
		cents
Germany	172,968	1.621
British-American	114,766	1.558
Other countries	52,311	1.368
Great Britain	133,546	1.027
United States	958,530	1.023
British Government	9,219	0.911
France	132,852	0.697
U. S. Government	50,472	0.226
Total and weighted average	1,624,664	1.084

It is evident, from what has now grown to be a substantial sample, that during the past thirteen years biological books from all over the world taken together have averaged to cost the American biologist very close to a cent a page, taking good, bad, and indifferent together. Furthermore it is plain that the sources of origin of these books fall into three fairly sharply defined groups relative to unit prices to the American buyer. In the first or relatively high priced group fall books in the British-American, Germany, and "Other countries" categories of origin. The primary reason why the British-American books fall in this category is because they carry an import duty charge, paid on the sheets manufactured in England but issued here by an American branch house. The next or medium priced group includes the United States, British Government, and Great Britain (commercial publishers). None of the books in this category carry import duty charges in the prices here tabulated, because in the case of the British books the English prices are used. Actually an American buying these books would have to pay duty. This would then, in fact, throw them into the same group as the British-American publications. Finally the third or relatively low price group includes books published in France and by the U. S. Government.

The reader should bear in mind that these reports are based on small samples of books in general and, for some countries, on small samples of the biological books published. He should therefore be cautious in applying conclusions drawn from this material to the general domain of book prices.

INDEX